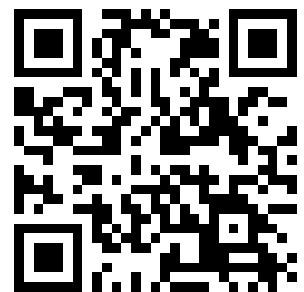


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guns each, killing 2 or even 3 geese at each shot, for they are very expert. Mr. Graham says, he has seen a row of Indians, by calling round a flock of geese, keep them hovering among them, till every one of the geese was killed. Every species of geese has its peculiar note or call, which must greatly increase the difficulty of enticing them.

Anas. 47. Albeola. 199. 18. The Red Duck. Faun. Am. Sept. 17. Edw. t. 100. Sarcelle de la Louisiane. Brisson vi. t. 41, f. 1. Severn river, N° 37 and 38. Fishing Birds.

The descriptions and figures answer very well with the male, except that the 3 exterior feathers are not white on the outside, but all dusky. The female is not described by any one of the ornithologists; and therefore deserves to be noticed, to prevent future mistake. The whole bird is dusky, a few feathers on the forehead are rusty, and some about the ears of a dirty white; the breast is grey, the belly and speculum in the wings white; the bill and legs are black. They visit Severn settlement in June, build their nests in trees, and breed among the woods, and near ponds; the weight of the female is 1 pound, its length 14 inches, and its breadth 21.

Anas. 48. Clangula. 201. 23. Golden Eye. Br. Zool. Faun. Am. Sept. 16. Severn river, N° 51.

These birds frequent lakes and ponds, and breed there: they eat fish and slime, and cannot rise off the dry land. The legs and irides are yellow; their weight is 2½ pounds, and their measure 19 inches in length, and 2 feet in breadth. The specimen sent is the male.

Anas. 49. Perspicillata. 201. 25. Black Duck. Faun. Am. Sept. 16. Edw. 155. Churchill river, N° 14.

This species is exactly described, and well drawn by Edwards. The Indians call it she-ke-supartem. It ought to come into the first division of Linnæus's ducks, "rostro basi gibbo," as its bill is really very unequal at the base.

Anas. 50. Glacialis. 203. 30, and Hyemalis, 202. 29. Edw. t. 156. Swallow-tail. Br. Zool. Faun. Am. Sept. 17. Churchill river, N° 12.

At Churchill river the Indians call this species, har-har-vey; it corresponds with Edwards's description and drawing, pl. 156, but differs much from Linnæus's inexact description of the *anas hymalis*, to which he however quotes Edwards. On the whole, it is almost without a doubt that the bird represented by Edwards, plate 280, and Br. Zool. folio, plate a, 7, and quoted by Linnæus for his *anas glacialis*, is the male, and that the bird figured by Edwards t. 156, and quoted by Linnæus for the *anas hyemalis*, is the female, of one and the same species. Linnæus mentions a white body, in his *anas hyemalis*, which in Edw. tab. 156, and in the society's specimen, is all brown and dusky, except the belly, temples, a spot on the back of the head, and the sides of the rump, which are white.

Linnaeus says, that the temples are black; in the specimen now sent over, and in Mr. Edwards's figure, which Linnaeus quotes, they are white; the breast, back, and wings, are not black as he says, but rather brown and dusky. A further proof, that Linnaeus's *anas glacialis* and *hyemalis* are the same, is that the feet in both t. 156 and 280 of Edwards are red, and the bill black, with an orange spot.

Anas. 51. *Crecca*. 204. 33. Varietas. Teal. Br. Zool. Faun. Am. Sept. 17. Severn river, N<sup>o</sup> 33, 34. Male and female.

This is a variety of the teal, for it wants the two white streaks above and below the eyes; the lower one indeed is faintly expressed in the male, which has also a lunated bar of white over each shoulder; this is not to be found in the European teal. This species is not very plentiful near Severn river; they live in the woods and plains near little ponds of water, and have from 5 to 7 young at a time.

Anas. 52. *Histrionica* 204. 35. Harlequin Duck. Faun. Am. Sept. 16. Edw. t. 99. This bird had no number fixed to it; it agrees perfectly with Edwards's figure.

Anas. 53. *Boschas*. 205. 40. Mallard Drake. Faun. Am. Sept. Br. Zool. Severn river, N<sup>o</sup> 39.

It is called stock drake at Hudson's Bay, and corresponds in every respect with the European one, upon comparison.

21. *Pelicanus*, Pelican. 54. *Onocrotalus* 251. 1. A variety. York fort.

This variety of the pelican, agrees in every particular with Linnaeus's oriental pelean (*pelecanus onocrotalus orientalis*), but has a peculiar tuft or fringe of fibres in the middle of the upper mandible, something nearer the apex than the base. This tuft has not been mentioned by any author, and is also wanting in Edwards's pelican, [t. 92, with which the society's specimen corresponds in every other circumstance. The *P. onocrotalus occidentalis*, Linn. or Edw. t. 93 American pelican, is very different from it: the chief differences are the colour, which in our Hudson's Bay bird is white, but in Edwards's is of a greyish brown; and the size, which in the white bird is almost double of the brown one. The quill feathers are black, and the shafts of the larger ones white. The alula, or bastard wing, is black. The bill and legs are yellow.

\* 22. *Colymbus*, Diver, 55. *Glacialis*. 221. 5. Northern Diver. Br. Zool. Faun. Am. Sept. 16. Churchill river, N<sup>o</sup> 8. called a Loon there.

This bird is well described and drawn in the British Zoology, in folio.

\* \* Grebe. 56. *Auritus*, a. 222. 8. Edw. 145. Eared Grebe. Faun. Am. Sept. 15. Severn river. N<sup>o</sup> 43.

This is exactly the bird drawn by Edwards, t. 145. The specimen sent over is a female. It differs much from our lesser crested grebe. Br. Zool. octavo 1, p. 396, and Br. Zool. illustr. plate 77, fig. 2, and Ed. 96. fig. 2. However, in,

both these works, it is considered only as a variety, or different in sex. Mr. Graham has the same opinion. It lives on fish, frequenting the lakes near the sea coast. It lays its eggs in water, and cannot rise off dry land. It is seen about the beginning of June, but migrates southward in autumn. It is called sekeep, by the natives. Its eyes are small, the irides red; it weighs 1 pound, and measures 1 foot in length, and one-third more in breadth.

23. *Larus*, Gull. 57. *Parasiticus*. 226. 10. Arctic Gull. Br. Zool. Faun. Am. Sept. 16. Edw. 148. 149. Churchill river, N<sup>o</sup> 15.

This species is called a man of war, at Hudson's Bay. It seems to be a female, by the dirty white colour of its plumage below; it agrees very well with Edwards's drawing, and that in the Br. Zool. illustr.

24. *Sterna*, tern. 58. *Hirundo* (variety), 227. 2. The greater tern. Br. Zool. Faun. Am. Sept.

The number belonging to this bird is lost, perhaps it is N<sup>o</sup> 17, from Churchill river, called 'a sort of gull, called egg-breakers, by the natives.' The feet are black; the tail is shorter and much less forked than that described and drawn in the Br. Zool. The outermost tail-feather also wants the black, which that in the British Zoology has. In other respects it is the same.

*XXX. Geometrical Solutions of Three Celebrated Astronomical Problems. By the late Dr. Henry Pemberton, F.R.S. Communicated by Matthew Raper, Esq., F.R.S. p. 434.*

*Lemma.*—To form a triangle with two given sides, that the rectangle under the sine of the angle contained by the two given sides, and the tangent of the angle opposite to the lesser of the given sides, shall be the greatest that can be.

Let the two given sides be equal to *AB*, and *AC* fig. 4, pl. 7: round the centre *A*, with the interval *AC*, describe the circle *CDE*, and produce *BA* to *B*; take *BF* a mean proportional between *BE* and *BC*, and erect the perpendicular *FG*, and complete the triangle *AGB*.

Here the sine of *BAG* is to the radius, as *FG* to *AG*; and the tangent of *ABG* to the radius, as *FG* to *FB*: therefore the rectangle under the sine of *BAG* and the tangent of *ABG*, is to the square of the radius, as the square of *FG*, or the rectangle *EFC*, to the rectangle under *AG* or *AC* and *FB*. But, *EB* being to *BF* as *BF* to *BC*, by conversion, *EB* is to *EF* as *BF* to *FC*, and also, by taking the difference of the antecedents and of the consequents, *EF* is to twice *AF* as *BF* to *FC*; and twice *AFB* is equal to *EFC*.

Now, let the triangle *BAH* be formed, where the angle *BAH* is greater than *BAG*. Here, the perpendicular *HI* being drawn, the rectangle under the sine of *BAH* and the tangent of *ABH*, will be to the square of the radius, as the rectangle *EIC*, to the rectangle under *AC*, *IB*. But *IF* is to *FB* as *2AFI* to *2AFB*, or

EFC; and  $2AFI$  is greater than  $AF^2 - AI^2$ ; also  $AF^2 - AI^2$  together with EFC, is equal to EIC; therefore by composition, the ratio of IB to BF is greater than that of EIC to EFC; and the ratio of  $AC \times IB$  to  $AC \times FB$  greater than that of EIC to EFC: also, by permutation, the ratio of  $AC \times IB$  to EIC greater than the ratio of  $AC \times FB$  to EFC. But the first of these ratios is the same with that of the square of the radius to the rectangle under the sine of BAH and the tangent of ABH; and the latter is the same with that of the square of the radius to the rectangle under the sine of BAG and the tangent of ABG; therefore the latter of these two rectangles is greater than the other.

Again, let the triangle BAK be formed, with the angle BAK less than BAG, and the perpendicular KL be drawn. Then the rectangle under the sine of BAK and the tangent of ABK, is to the square of the radius, as the square of KL to the rectangle under AC, BL. Here, FL being to FB as  $2AFL$  to  $2AFB$  or EFC, and  $2AFL$  less than  $AL^2 - AF^2$ , by conversion, the ratio of LB to FB will be greater than the ratio of ELC to EFC; therefore, as before, the rectangle under the sine of BAG and the tangent of ABG is greater than that under the sine of BAK and the tangent of ABK.

*Corol. 1.* BF is equal to the tangent of the circle from the point B; therefore BF is the tangent, and AB the secant, to the radius AC, of the angle, whose cosine is to the radius as AC to AB. Therefore AF is the tangent, to the same radius, of half the complement of that angle; and AF is also the cosine of the angle BAG to this radius.

*Corol. 2.* The sine of the angle composed of the complement of AGB, and twice the complement of ABG, is equal to 3 times the sine of the complement of AGB. Let fall the perpendicular AH, (fig. 5), cutting the circle in I; continue GF to K, and draw AK. Then  $BF^2 = EBC = GBL$ . Therefore  $GB : BF :: BF : BL$ , and the triangles GBF, FBL are similar. Consequently FL is perpendicular to GB, and parallel to AH; whence GH being equal to HL, GM is equal to MF, and MK equal to 3 times GM.

Now the arc  $IK = 2IC + GI$ ; and the angle  $IAK = 2IAC + GAI$ ; also GM is to MK as the sine of the arc GI to the sine of the arc IK, that is, as the sine of the angle GAI to the sine of the angle IAK. Therefore the sine of the angle IAK ( $= 2IAC + GAI$ ) is equal to 3 times the sine of the angle GAI; but GAI is the complement of AGB, and IAC the complement of ABG.

*Corol. 3.* If (fig. 6) any line BN be drawn to divide the angle ABG, and AN be joined, also AO be drawn perpendicular to BN, and continued to the circle in P, the sine of the angle composed of NAP and  $2PAC$  will be less than 3 times the sine of the angle NAP. Draw NQB perpendicular to AB, cutting AP in S; join AR, and draw QT perpendicular to BN, and parallel to AO; then  $BQ^2 = NBT$ . But  $BQ^2$  is greater than the rectangle EBC, that is, greater than the rectangle NBV,

under the 2 segments of the line BN drawn from B, to cut the circle in N and V; therefore TB is greater than VB, and NO greater than OT. Consequently NS is greater than SO. Hence RS is less than 3 times NS; and therefore the sine of the angle PAR ( $= \text{NAP} + 2\text{PAC}$ ) is less than 3 times the sine of NAP.

PROB. I.—*To find in the Ecliptic the Point of Longest Ascension.*

ANALYSIS.—Let (fig. 7) ABC be the equator, ADC the ecliptic, BD the situation of the horizon, when D is the point of longest ascension. Let EFG be another situation of the horizon. Then the ratio of the sine of EB to the sine of FD is compounded of the ratio of the sine of BG to the sine of GD, and of the ratio of the sine of AB to the sine of AF; but the angles B and E being equal, the arcs EG, GB together make a semicircle; and, by the approach of EG towards GB, the ultimate magnitude of BG will be a quadrant, and the ultimate ratio of EB to FD will be compounded of the ratio of the radius to the sine of DG (that is, the cosine of BD) and of the ratio of the sine of AB to the sine of AD. Draw the arc DH perpendicular to AB. Then, in the triangle BDH, the radius is to the cosine of BD, as the tangent of the angle BDH to the co-tangent of HBD. Also, in the triangle BDA, the sine of AB is to the sine of AD, as the sine of the angle BDA (or BDC) to the sine of ABD; therefore the ultimate ratio of BE to DF is compounded of the ratio of the tangent of BDH to the cotangent of ABD, and of the ratio of the sine of BDC to the sine of ABD; which two ratios compound that of the rectangle under the tangent of BDH and the sine of BDC, to the rectangle under the cotangent and the sine of the given angle ABD.

But when D is the point of longest ascension, the ratio of BE to DF is the greatest that can be; therefore then the ratio of the rectangle under the tangent of BDH and the sine of BDC, to the given rectangle under the cotangent and sine of the given angle ABD, must be the greatest that can be; and consequently the rectangle under the tangent of BDH and the sine of BDC, must be the greatest that can be.

In the triangle BDA, the sine of BDH is to the sine of HDA, as the cosine of ABD to the cosine of BAD. Now, in the preceding lemma, let the angle BAG of the triangle AGB be equal to the spherical angle BDC: then will the sum of the angles ABG, AGB be equal to the spherical angle BDA. And, if AG in the triangle AGB, be to AB, as the cosine of the spherical angle DBA to the cosine of DAB, that is, as the sine of BDH to the sine of HDA, the angle ABG, in the triangle, will be equal to the spherical angle BDH; and the angle AGB, in the triangle, equal to the spherical angle HDA. Therefore, by the first corollary of the lemma, that the rectangle under the tangent of the spherical angle BDH and the sine of BDC, be the greatest that can be, the cosine of BDC must be equal to the tangent of half the complement of the angle, whose cosine is to the radius, as AG

to AB, in the triangle, or as the cosine of the spherical angle ABD, to the cosine of the spherical angle BAD.

If IK be the situation of the horizon, when the solstitial point is ascending, in the quadrantal triangle AIK, the cosine of KIC is to the radius, as the cosine of IKA (= DBA) to the cosine of IAK. Therefore the cosine of BDC, when D is the point of longest ascension, is equal to the tangent of half the complement of the angle which the ecliptic makes with the horizon, when the solstitial point is ascending.

But the sine of the angle composed of DAB and twice ABD, must be less than 3 times the sine of the angle BAD. In the spherical triangle ABD, the angles BAD, ABD together exceed the external angle BDC. Therefore, in the 3d corol. of the lemma, let the angle BAN be equal to the sum of the spherical angles BAD, ABD: but here, AN is to AB as the cosine of the spherical angle ABD to the cosine of BAD; and AN is also to AB as the sine of ABN to the sine of ANB, that is, as the cosine of BAP to the cosine of NAP; consequently, since the angle BAN is equal to the sum of the spherical angles BAD, ABD, the angle NAP is equal to the spherical angle BAD, and the angle BAP equal to the spherical angle ABD; but the sine of the angle composed of NAP and twice FAB is less than three times the sine of NAP: therefore the sine of the angle composed of the spherical angle BAD and 2ABD will be less than three times the sine of the angle BAD; otherwise no such triangle DBA, as is here required, can take place, but the point A will be the point of longest ascension.

If the sine of the angle A be greater than  $\frac{1}{4}$  of the radius, the point A can never be the point of longest ascension; but when the sine of this angle is less, the angle compounded of BAD and twice ABD, may be greater or less than a quadrant; and therefore the magnitude of the angle ABD, that A be the point of longest ascension, is confined within 2 limits, of which the double of one added to the angle A, as much exceeds a quadrant, as the double of the other added to that angle falls short of it; therefore double the sum of those two angles, together with twice A, makes a semicircle; and the single sum of those two angles added to A makes a quadrant.

PROB. II.—*To find when the Arc of the Ecliptic Differs Most from its Oblique Ascension.*

ANALYSIS.—If (fig. 8) BD be the situation of the horizon, when CD differs most from CB, as before, the ultimate ratio of BE to DF, will be compounded of the ratio of the radius to the sine of DG (or the cosine of DB) and of the ratio of the sine of CB to the sine of CD: but when CD differs most from CB, BE and DF are ultimately equal; therefore then the cosine of BD is to the radius as the sine of CB to the sine of CD.



Draw the arc  $CHI$  of a great circle, that  $DH$  be equal to  $DB$ ; then,  $BH$  being double  $BD$ , half the sine of  $BH$  is to the sine of  $BD$  or  $DH$ , as the cosine of  $BD$  to the radius, therefore half the sine of  $BH$  is to the sine of  $DH$  as the sine of  $CB$  to the sine of  $CD$ ; but the sine of the angle  $BCH$  is to the sine of  $BH$  as the sine of the angle  $CHB$  to the sine of  $CB$ ; whence, by equality, half the sine of  $BCH$  is to the sine of  $DH$  as the sine of  $CHB$  to the sine of  $CD$ : but as the sine of  $CHB$  to the sine of  $CD$ , so, in the triangle  $CHD$ , is the sine of  $DCH$  to the sine of  $HD$ : consequently the sine of  $DCH$  is equal to half the sine of  $BCH$ . Hence, the difference of the angles  $BCH$ ,  $DCH$  being given, those angles are given, and the arc  $CHI$  is given by position.

Further, in the triangle  $BCH$ , the base  $BH$  being bisected by the arc  $CD$ , the sine of the angle  $CHD$  is to the sine of the given angle  $CBD$ , as the sine of the given angle  $HCD$  to the sine of the given angle  $BCD$ ; therefore the angle  $CHB$  is given: because that in the triangle  $CBH$  all the angles are given. The sum of the sines of the angles  $BCH$ ,  $DCH$  is to the difference of their sines, as the tangent of half the sum of those angles to the tangent of half their difference; therefore the tangent of half the sum of  $BCH$ ,  $DCH$  is 3 times the tangent of half  $BCD$ .

In (fig. 9) the isoscles triangle  $ABC$ , let the angle  $BAC$  be equal to the spherical angle  $BCD$ , and let  $AE$  be perpendicular to  $BC$ ; also,  $CF$  being taken equal to  $CB$ , join  $AF$ : then  $EF$  is equal to 3 times  $EB$ ; and as  $EF$  to  $EB$ , so is the tangent of the angle  $EAF$  to the tangent of  $EAB$ ; but  $EAB$  is equal to half the spherical angle  $BCD$ : therefore the angle  $EAF$  is equal to half the sum of the spherical angles  $BCD$ ,  $BCH$ ; and consequently the angle  $CAF$  equal to the spherical angle  $DCH$ . Here  $AF$  is to  $CF$  as the sine of the angle  $ACF$  to the sine of  $CAF$ : and  $CB$  is to  $AB$  as the sine of the angle  $BAC$  to the sine of  $ACB$ : therefore,  $CF$  being equal to  $CB$ , and the sine of  $ACF$  to the sine of  $ACB$ , by equality,  $AF$  is to  $AB$  as the sine of the angle  $BAC$  to the sine of  $CAF$ , that is, as the sine of the spherical angle  $BCD$  to the sine of the spherical angle  $DCH$ .

Let (fig. 10) the triangle  $AGB$  have the angle  $ABG$  equal to the spherical angle  $CBD$ , and the side  $AG$  equal to  $AF$ . Then,  $AG$  is to  $AB$  as the sine of the spherical angle  $BCD$  to the sine of the spherical angle  $DCH$ , that is, as the sine of the spherical angle  $CBH$  to the sine of the spherical angle  $CHB$ ; but  $AG$  is to  $AB$  also as the sine of the angle  $ABG$  to the sine of  $AGB$ ; therefore, the angle  $ABG$  being equal to the spherical angle  $CBH$ , the angle  $AGB$  is equal to the spherical angle  $CHB$ : and also, when the angle  $ABG$  is greater than  $ABF$ , that is, when the spherical angle  $CBH$  is greater than the complement of half  $BCD$ , the 3 angles  $ABG$ ,  $AGB$  and  $BAC$  together exceed 2 right angles.

Hence, (fig. 11) towards the equinoctial point  $c$ , where the angle  $CBD$  is obtuse, a situation of the horizon, as  $BD$ , may always be found, wherein  $CD$  more exceeds  $CB$  than in any other situation: and when the acute angle  $DBA$  is greater

than the complement of half  $BCD$ , another situation of the horizon, as  $KLM$ , may be found, toward the other equinoctial point  $A$ , wherein the arc of the ecliptic  $CK$  will be less than the arc of the equator, and their difference be greater than in any other situation. But if the angle  $CBA$  be not greater than the complement of half  $BCD$ , the arc of the ecliptic, between  $c$  and the horizon, will never be less than the arc of the equator, between the same point  $c$  and the horizon. In the two situations of the horizon, the angles  $CHB$  and  $KMA$  are equal.

*Schol. 1.* To find the point in the ecliptic, where the arc of the ecliptic most exceeds the right ascension, is a known problem: that point is, where the cosine of the declination is a mean proportional between the radius and the cosine of the greatest declination.

In the preceding figure, supposing the angle  $CBD$  to be right, then, because when  $CD$  most exceeds  $CB$ , the cosine of  $BD$  is to the radius as the sine of  $CB$  to the sine of  $CD$ , and, in the triangle  $CBD$ , the sine of  $CB$  is to the sine of  $CD$  as the sine of the angle  $CDB$  to the radius, also the sine of  $CDB$  is to the radius as the cosine of  $BCD$  to the cosine of  $BD$ ; therefore the cosine of  $BD$  is to the radius as the cosine of the angle  $BCD$  to the cosine of the same  $BD$ , and the cosine of  $BD$  is a mean proportional between the radius and the cosine of  $BCD$ .

*Schol. 2.* In any given declination of the sun, to find when the azimuth most exceeds the angle which measures the time from noon, is a problem analogous to the preceding.

PROB. 3.—*The Tropic found, by Dr. Halley's method,\* without any consideration of the parabola.*

The observations are supposed to give the proportions between the differences of the sines of 3 declinations of the sun near the tropic; but the sine of the sun's place is in a given proportion to the sine of the declination; therefore the same observations give equally the proportion between the differences of the sines of the sun's place, in each observation.

Now, (fig. 12), let  $ACE$  be the ecliptic,  $AE$  its diameter between  $\gamma$  and  $\epsilon$ , and its centre  $F$ ; let  $B, C, D$  be 3 places of the sun;  $BG, CI, DH$  the sines of those places respectively. Draw  $CK, BL$  parallel to  $AE$ , which may meet  $HD$  in  $N$  and  $M$ . Then, by the observations, the ratio of  $DM$  to  $DN$  is given. Therefore, if  $BD$  be drawn to meet  $KL$  in  $O$ , the ratio of  $BD$  to  $OD$  is given; and the ratio of  $BD$  to  $DC$  is also given, these being the chords of the given angles  $BFD, CFD$ : hence the ratio of  $CD$  to  $DO$ , in the triangle  $CDO$ , is given; and consequently the angle  $COD$  will be given: which angle is the distance of the tropic from the middle point of the ecliptic between  $B$  and  $D$ : for,  $FPR$  being perpendicular to  $OC$ , and  $FQS$  perpendicular to  $DB$ , the angle  $QFP$  is equal to  $QOP$ , the points  $O, P, Q, R$ , being in a circle.

\* Vide Phil. Trans. N<sup>o</sup> 215.

*The Calculation.*— $DN : DM :: S. \frac{1}{2} BFD : S. \frac{1}{2} CFD :: \text{rad. } t. < \chi ; \text{rad.} :: t.$   
 $(< \chi \infty 45^\circ) :: t. \frac{1}{2} BFC : t. \frac{1}{2} COD \infty \frac{1}{2} DCO.$  If  $\chi > 45^\circ$ ,  $< COD > DCO$ ; and  
 if  $\chi < 45^\circ$ ,  $< COD < DCO.$

If the intervals between the observations be so small, that the sines differ not much from the arches, the arches BC, CD may be counted in time, and the calculation may be abbreviated thus:  $DM : DN :: \text{arc. } BD : z$  (for DO);  $DC + z : 2DC :: \frac{1}{2} BC : SR.$ , or  $DM \times DC + DN \times BD : DM \times DC :: \frac{1}{2} BC : SR.$

*XXXI. On the Digestion of the Stomach after Death. By John Hunter,\*  
 F. R. S., and Surgeon to St. George's Hospital. p. 447.*

Reprinted in Mr. Hunter's Observations on the Animal Economy.

\* Mr. John Hunter is a remarkable instance of the eminence which the human intellect is sometimes capable of attaining in particular pursuits of science, without the previous aid of a good general education, and even after a large portion of the youthful period of life has been suffered to pass away in vacancy and inattention.

He was brother to the celebrated Dr. William Hunter, (an account of whom is inserted in the 8th volume of these Abridgments) and was born at Long Calderwood in 1728. His father dying when he was about 10 years old, he was left to the care of his mother, who, in consequence of his dislike to school, suffered him to remain at home in idleness; so that from the time of his father's death to the age of 20, the cultivation of his mind appears to have been neglected, and he was, without any regular occupation or pursuit.

At length, however, having heard much of his brother's celebrity and success in London, he expressed a desire to study anatomy. This desire was readily seconded by Dr. W. Hunter, to whose house Mr. J. H. accordingly came in 1748. It was now found that Mr. J. H. possessed talents, which only wanted a proper stimulus and direction. He soon became competent to the office of assistant dissector. While he was engaged in anatomical pursuits, he did not lose sight of surgery; a knowledge of which he acquired by attending Chelsea, Bartholomew, and St. George's hospitals; to the last of which he became house-surgeon's pupil in 1754, and house-surgeon in 1756. The year before his brother admitted him to a partnership in the anatomical lectures. His health becoming much impaired by his close application to dissections, and the making of anatomical preparations, he was advised to go abroad; and accordingly in 1760 he went as surgeon on the staff with the army to Belleisle, and afterwards to Portugal. It was in this situation that he acquired his knowledge of gunshot wounds. In 1763 he returned to London and resumed his labours in anatomy and surgery. In 1767 he was chosen F. R. S. and some years after the same honour was conferred on him by the Royal Society of Medicine and the Royal Academy of Surgery at Paris. In 1769 he was elected one of the surgeons to St. George's hospital. Some years after he was appointed surgeon extraordinary to his Majesty, and inspector general of hospitals, and surgeon general to the army.

Previously to the attainment of these honours, he had distinguished himself by various papers inserted in the Phil. Trans., relating to anatomy and physiology; also by some publications of a larger kind, such as his Natural History of the Teeth. It was not till a later period that he published his Treatise on the Venereal Disease, and his Observations on the Animal Economy; which last work consists chiefly of papers which had before been printed in the Phil. Trans. His Treatise on the Blood; Inflammation, and Gunshot Wounds, did not appear till after his decease; being edited by his relation Mr. Meade, and accompanied with a biographical account of the author; from which account most of the particulars abovementioned have been extracted.

Mr. J. Hunter contributed largely to the advancement of physiology and comparative anatomy,

*XXXII. Experiments and Observations on the Waters of Buxton and Matlock, in Derbyshire. By T. Percival,\* of Manchester, M. D., F.R.S. p. 455.*

Reprinted in this author's collected works.

not only by his lectures and writings, but also by the number and variety of preparations of which his museum consisted. The formation of this collection, which subjected him to vast labour and expence, was a favourite and principal object of his life. It was (as has been well remarked by Mr. Home) a grand attempt to expose to view the gradations of nature, from the most simple state in which life is found to exist, up to the most perfect and most complex of the animal creation,—man himself. The public will bear with pleasure that this valuable collection has recently (1807) been purchased by government and presented to the College of Surgeons for their use.

The various pursuits relative to anatomy, physiology, and surgery, in which Mr. J. Hunter was engaged, were followed with so much assiduity as to prove injurious to his health. After several previous attacks of the gout, he was seized in 1773 and 1776, and at irregular periods, for some years afterwards, with violent and alarming symptoms, apparently spasmodic, but proceeding (as it afterwards appeared) from an organic affection of the heart: and in one of these attacks he died suddenly, while he was at St George's hospital, in Oct. 1793, being then in the 65th year of his age.

In all his writings Mr. J. Hunter was truly original, deriving his knowledge, not from books (for he rarely consulted them) but from actual experiment and observation. Whatever may be thought of some of his opinions, we cannot sufficiently admire that talent for investigation, by which he was enabled to make the most interesting discoveries relative to the animal economy; discoveries which give him a just claim to be placed in the very first rank of those philosophers, who, in this country, have particularly contributed to the advancement of comparative anatomy and physiology.

\* The following particulars concerning this distinguished medical and moral writer, are taken from the *Biographical Memoirs* prefixed to the edition of his works in 4 vols. 8vo. recently published (1807) by his son.

Dr. Thos. Percival was born in 1740 at Warrington, where he received his grammatical education. At the age of 21, he went to study physic at Edinburgh. He afterwards removed to Leyden, at which university he took his degree of M. D., in 1765, and visited Paris, before he returned to England. After 2 years spent in his native town, he at length decided on removing to Manchester, where he established himself in 1767 as a practising physician. About this time he published the first volume of his *Essays, Medical and Experimental*, some of which had been previously inserted in the *Transactions of the Royal Society*, of which he had been elected a member 2 years before. From this time he continued to extend his reputation as an author by various publications on subjects connected with philosophy, physic, and morality. For his ingenious communications in these several departments of science, he was elected a member of the Royal Societies of Paris and Edinburgh, and of the American Philosophical Society, &c. For many years preceding his death, Dr. P. was deprived of his eye-sight, in consequence of which he was ever afterwards obliged to employ an amanuensis. He was also subject to periodical attacks of severe head-ache; but his habitual serenity of mind was never discomposed in the slightest degree by these bodily afflictions. He died in 1804, being then in the 64th year of his age. On the monument, erected to his memory, in Warrington church, is engraved an elegant Latin inscription, written by the Rev. Dr. Samuel Parr.

Of Dr. P.'s writings on physic, the principal are his *Essays* beforementioned, and his *Medical Ethics*: those on philosophy consist of *Dissertations* inserted in the *Trans. of the R. S.*, and in the *Memoirs of the P. S. of Manchester*; and among those which relate to Morality, not the least valuable are the *Essays* entitled "A Father's Instructions," &c. These, with several other compositions, constitute the 4 vols. of his works collected and edited by his son.

It was to Dr. P.'s fondness for literary and scientific intercourse that the P. S. of Manchester, (over

*XXXIII. Some Account of a Body lately found in Uncommon Preservation, under the Ruins of the Abbey at St. Edmund's Bury, Suffolk; with some Reflections on the Subject. By Charles Collignon, M. D., F. R. S., and Prof. of Anat. at Cambridge. p. 465.*

In February 1772, some workmen, digging among the ruins of the above abbey, discovered a leaden coffin, supposed, from some circumstances, to contain the remains of Thos. Beaufort, Duke of Exeter, uncle to king Henry the 5th. As it certainly was buried before the dissolution of the abbey, it must have been there between 2 and 300 years. It was found near the wall, on the left-hand side of the choir of the chapel of the blessed Virgin; not inclosed in a vault, but covered over with the common earth. On examining the appearance of the body, the following circumstances were remarkable, as communicated by an ingenious surgeon, on the spot, Mr. Thomas Cullum.

“The body was inclosed in a leaden coffin, surrounding it very close, so that you might easily distinguish the head and feet. The corpse was wrapped round with 2 or 3 large layers of cere-cloth, so exactly applied to the parts, that the piece, which covered the face, retained the exact impression of the eyes and nose. The dura mater was entire. The brain was of a dark ash-colour, with some remaining appearance of the medullary part. The coats of the eye were still whole, and had not totally lost their glistening appearance. There was about half a pint of a bloody black water in the thorax; and a mass that seemed to be part of the lungs. The pericardium and diaphragm were quite entire. The abdominal viscera had been taken out very clean, and the integuments and muscles stuck very close to the vertebræ of the back. This cavity looked fresher than that of the thorax. I cut into the *psoas magnus*; where there were evident marks of red muscular fibres. The other muscles had lost all their red colour, and were become of a dark brown. The tendons were still strong and retained their natural appearance. The hands, which are preserved in spirits, retain the nails. There were some very small holes in the coffin, out of which had run some bloody water, of an offensive smell. All the principal blood-vessels must have been cut through, in taking out the abdominal viscera: and if no ligature was made on the vessels, their contents would escape, particularly as assisted by the pressure of the cere-cloth, which is of considerable weight, and doubtless put on hot. This fluid running out of the coffin, on its being moved, might occasion the suspieion of the body being put in pickle.”

We have undoubted accounts of bodies found very little changed after long

which he presided 20 years) owed its origin. He was also a chief promoter of another literary institution, the Manchester Academy, which, however, did not long flourish. And he exerted himself with much assiduity in support of the Fever Wards, and other measures that have been adopted at Manchester within these few years, for stopping the progress of infectious fevers.

interment, where there was no appearance of any art having been used. And doubtless some constitutions are more prone to putrefaction after death than others; these circumstances may be dependant on age, sex, and last disease; to which predisposing causes, thus attending persons to the grave, are to be added the soil and situation in which they are deposited. Could we be masters of all these particulars, in the few dead bodies hitherto discovered greatly free from the usual putrefaction, it would lead perhaps to the probable cause of the phenomenon, and point out a proper method of imitation. And till that is done, it is difficult to know how much merit is to be assigned to the art or mystery of embalming, and how much to the power of natural causes.

*XXXIV. A Letter from Richard Pulteney, M. D., F. R. S., to Wm. Watson, M. D., F. R. S., concerning the Medicinal Effects of a Poisonous Plant exhibited instead of the Water Parsnep. p. 469.*

Some circumstances having lately come to my knowledge (says Dr. P.) relating to the effect of a poisonous plant, I thought them rather too remarkable not to merit further notice; and, I address them to you with the more propriety, as you have already laid before the public some observations\* concerning the deleterious qualities of the plant in question, which holds a distinguished place among the poisonous ones that are indigenous in Britain.

Mr. H——n, an attorney of this place, now upwards of 40, at the age of 15, began to be affected, after taking cold on violent exercise, as he thinks, with what is usually called a scorbutic disorder; which showed itself more particularly on the outsides of his arms, about the elbows, and on the outsides of his legs, from the knees to the ancles, as well as in botches on other parts of his body. It had the appearance of a dry branny scab or scurf, which every night fell off, more or less in scales, as is usual in leprous cases. At times it pushed out more than usual, and thickened the integuments of the limbs considerably, after which the separation of scales would become very abundant.

For several years past he had been trying a variety of things commonly recommended in such cases, particularly the quack medicine known by the name of Maredant's drops, which he continued for near a year, without finding the least sensible relief: also an electuary of flos sulphuris and cremor tartari, which he had persevered in for near 3 years, without finding any other alteration, than that of its preventing costiveness, to which he was habitually subject. In the winter 1770, this disorder increased very rapidly, without being able to assign any reason, from any accident that had happened to him, or from any irregularity of his own in point of regimen, in which he was always very exact. At this

\* See Phil. Trans. vol. 44, p. 227, and vol. 50, p. 856, of these Abridgments, vol. ix. p. 256, and vol. xi. p. 311.

time, besides the farther spreading of the irruption itself, the integuments of the legs thickened very much, and the limbs swelled to such a degree, as to render him unable to walk. The quantity of branny scurf and scales thrown off, at this time, was very great; he says, "handfuls might have been taken out of his bed every morning."

In this unhappy situation, even loathsome to himself, it was recommended to him to take the juice of water parsnep, in the quantity of one common table-spoonful every morning, fasting, mixed with 2 spoonfuls of white mountain wine. Accordingly, about the middle of January 1771, he procured a half-pint phial of what was so called, by means of the person who had recommended it, and who had assured him that he had been greatly relieved, in a similar disorder, by it. The first spoonful he took did not begin to give any great uneasiness for 2 hours, but after that time, his head began to be affected in a very extraordinary manner; a violent sickness soon succeeded, and violent vomiting; and, after he was put to bed, there came on cold sweats, and a very strong and long continued rigor, so that the people about him thought him dying for some time; but, in a few hours, all these symptoms wore off.

Such, however, had been the inveteracy of his disorder, and so strong his desire to find relief, that he determined not to desist; and, after having omitted his medicine for one day, he repeated it, in nearly the same dose, and with similar effects as to sickness and vomiting, though the uncommon sensation in his head, and the succeeding rigor, were by no means so violent. He had resolution enough to continue this dose every other morning, for more than a fortnight, and then reduced it to 3 tea-spoonfuls, which was just the half of the first dose.

Before he had taken this juice one month, he was sensible of a very great change for the better; encouraged therefore by these appearances, he persevered in its use till the middle of April, by which time his skin, though not quite cleared, yet had ceased to throw off any more scurf, was become soft, clean, and well conditioned, and, as he has repeatedly assured me, he got then into a much better conditioned state than he had experienced for many years before. From first to last, this juice never purged him; though he says, even in its reduced dose, it never failed to occasion a dizziness of the head, a nausea and sickness, which were not unfrequently succeeded by a vomiting, that always instantly relieved his head.

From the middle of April to the middle of June, he desisted from the use of the juice, but, in its stead, drank every morning for breakfast, the infusion of the leaves of the same plant, which, he says, is like common bohea tea. The infusion seldom occasioned nausea, or sickness, but always brought on a small

degree of vertigo, and in a slight manner produced the effects of intoxication from liquor.

In June he went to Harrowgate, as he had designed in the summer before. On first drinking and bathing there, he thought himself worse; and his eruptions having gradually increased during the 2 months that he staid in that place, he was convinced that those waters were of no real service to him. On his coming home he returned to the use of the infusion, and he assures me, that he again found, even by that weak preparation, a very speedy alteration for the better. From that time he continued it ever since, till his stock of the herb was exhausted; his skin is now so very little affected, that he has but here and there, on his arms and legs, a very small appearance of his disorder.

On questioning him as to the sensible qualities of this medicine, he says again, that he particularly remembers that it never once purged him; not even the first dose, which had so nearly poisoned him. He does not think that it increased the sensible perspiration, but is convinced that it was diuretic; and adds, that he thinks it occasioned, besides the increased flow of urine, a copious sediment in it, and which he believes was always wanting before. This is the plain narrative of the fact. He has assured me that no medicine or regimen, among the great variety that he has tried, ever had any sensible effect on his disorder before; and that nothing but the very early and sensible relief he experienced from this juice, could have induced him to persevere in its use, under such uneasy feelings as it never failed to produce. Indeed, he makes nothing of the lighter effects of the infusion, from which however he thinks he has likewise reaped no small benefit.

This case, the nature and inveteracy of his disorder being well known among his neighbours, was much talked of, and raised the curiosity of many people. When I first heard of it, and was informed of the smallness of the dose, and its virulent operation, I could scarcely doubt that the juice of some other plant had been administered instead of that of the water parsnep, which we know to be a safe and harmless vegetable; medical writers having directed its juice to be drunk, even to the quantity of 4 ounces for a dose: and as I know the oenanthe crocata, henlock dropwort, to be exceedingly plentiful in this country, so much as to be more easily procured than the water parsnep itself; I thought it probable that that plant had been used in its stead. On getting a specimen, it appeared that this had been indeed the case; as also, on further inquiry, that it was the juice of the root only, and not of the leaves and stalks, that had been administered. I might here observe, that the expression from the root is not to be depended on after the plant is advanced towards its flowering state, as the root then becomes light, spongy, and almost destitute of juice.

W. S.—Mr. H—— is desirous that it should be known, that he “tried very



*XXXV. Experiments on two Dipping Needles, made after a Plan of the Rev. Mr. Mitchel, F. R. S., Rector of Thornhill, and executed for the Board of Longitude by Mr. Edw. Nairne, of Cornhill, London. p. 476.*

The magnetic needles were 12 inches long, and their axes, the ends of which were of gold allayed with copper, rested on friction wheels of 4 inches diameter, each end on 2 friction wheels, which wheels were balanced with great care. The ends of the axes of the friction wheels were likewise of gold allayed with copper, and moved in small holes made in bell-metal; and opposite the ends of the axes of the needles, and the friction wheels, were flat agates, finely polished. Each magnetic needle vibrated in a circle of bell-metal, divided into degrees and half degrees, and a line passing through the middle of the needle to the ends pointed to the divisions. The minutes set down in the experiments were, by estimation, as the third of half a degree is counted 10 minutes. The instruments were carefully placed, so that the needles vibrated exactly in the magnetic meridian. The 2 needles were nearly balanced before they were made magnetical; but, by a curious contrivance of Mr. Mitchell, of a cross fixed on the axes of the needles (on the arms of which were cut very fine screws, to receive small buttons, that might be screwed nearer to or farther from the axis) the needles could be adjusted both ways, to a great nicety, after they were made magnetical, by reversing the poles, and changing the sides of the needle.

First set of experiments made April 21, 1772, by  
Edw. Nairne, at his house, N<sup>o</sup> 20, Cornhill.

Second set of experiments, with that side of the instrument to the east, which was to the west in the first observation.

**Here the ends of the axis touched the agates**

Third set of experiments, in which the poles of the needle were reversed, but the same side of the instrument to the east, as in the second set of experiments, and the needle rather more magnetical, being touched with a larger set of magnets.

{	72°	20'
	72	20
	72	20
	72	20
	72	20
	72	20
	72	10
	72	15
	72	45
	72	45
	72	5
	72	0
{	72	30
	72	30
	72	30
	72	30
	72	30
	72	30

Fourth set of experiments, viz. the same side of	72° 10'
the instrument to the east, as in the first set of experiments.	72 10
	72 15
	72 10
	72 10
	72 10

Fifth experiment, viz. the same end of the needle made north, as in the first set of experiments, and also the same side of the instrument to the west, as in the first set of experiments, 72° 20'.

Experiments made April 22, 1772, with the other	72° 15'
dipping needle, the instrument being put in the same	72 10
place, and with great care, in the magnetic meridian,	72 20

the needle pointed as annexed. On the 2d of these, the poles of the needle changed. And in the 3d, the side of the instrument to the east, which in the first observation was to the west.

Lest any thing magnetical should have affected the	72° 10' or 15'
needle in Mr. Nairne's house, he took this instrument,	72 20
and placed it in the middle of a large room	72 30
belonging to the London Assurance in Birchin-lane,	72 10

and then the needle pointed as annexed. At the 3d of these, the poles of the needle changed. And at the 4th, the side of the instrument to the east, which in the first observation was to the west.

The dipping needle brought back to Mr. Nairne's, and put in the same place as before, stood at 72° 10' +.

In the foregoing experiments, the needle was raised to an horizontal position, and left to vibrate. It was between 8 or 9 minutes before the vibration ceased. The needle brought to an horizontal position, and one grain and a half laid on the extremity of the south end, was not sufficient to keep it in that position; but the north end pointed to 35° 30'. One grain and three quarters laid on the extremity of the south end of the needle, was more than sufficient to keep it in the horizontal position, the south end then pointing 6° 45' below 0.

END OF THE SIXTY-SECOND VOLUME OF THE ORIGINAL.

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*I. Discovery of the Manner of making Isinglass in Russia; with a particular Description of its Manufacture in England, from the Produce of British Fisheries. By Humph. Jackson,\* Esq., F.R.S. Anno 1773. Vol. LXIII. p. 1.*

All authors, who have hitherto delivered processes for making ichthyocolla, fish-glue or isinglass, have greatly mistaken both its constituent matter and pre-

paration. To prove this assertion, it may not be improper to recite what Pomet says on the subject, as he appears to be the principal author whom the rest have copied. After describing the fish, and referring to a cut engraven from an original in his custody, he says: "As to the manner of making the isinglass, the sinewy parts of the fish are boiled in water, till all of them be dissolved that will dissolve; then the gluey liquor is strained, and set to cool. Being cold, the fat is carefully taken off, and the liquor itself boiled to a just consistency, then cut to pieces, and made into a twist, bent in form of a crescent, as commonly sold, then hung on a string, and carefully dried." From this account, it might be rationally concluded that every species of fish which contained gelatinous principles would yield isinglass: and this seems to have given rise to the hasty conclusions of those who strenuously vouch for the extraction of isinglass from sturgeon; but as that fish is easily procurable, the negligence of ascertaining the fact by experiment seems inexcuseable. Every traveller, as well as author, who mentions isinglass, observes that it is made from certain fish found in the Danube and rivers of Muscovy. Willughby and others inform us, that it is made of the sound of the beluga; Caspar Newman, that it is made of the huse germanorum and other fish, which he has seen frequently sold in the public markets of Vienna. These circumstances make it appear the more extraordinary, that a perfect account of the manufacture of such an essential article of commerce should remain so long unrevealed.

In Mr. J.'s first attempts to discover the constituent parts and manufacture of

\* Mr. Jackson died at Tottenham, June 29, 1801, at 84 years of age, where it is said he kept by him for some time before his death, a patent coffin to be interred in, and used at times to lie down in it, to show his acquaintances how it fitted him. Mr. J. kept a chemist's shop about Tower hill, London, where it seems speculating on schemes how at once to make a great fortune, he fell on that of brewing porter by certain drugs substituted as materials instead of malt and hops. With these he set up as a general instructor of the brewers, to initiate them into these new mysteries, for saving malt and hops, by giving private lessons in the art, at an enormous premium. This art it seems they have, in most instances, practised ever since in so extensive a manner, as to have produced a general complaint, that the ancient national malt liquor is miserably degenerated, with universal execrations on the memory of the man who could be so wicked as to introduce a practice, in consequence of which the natural beverage of the country has been ruined for ever. Among other pupils of Mr. J. was the late Mr. Thrale, the great brewer in the Borough, from whom alone it seems this charlatan extracted an ample fortune, as mentioned by Mr. T.'s widow, now Mrs. Piozzi, in her anecdotes of the life of Dr. Johnson,

After having by such means, in a short time, amassed an immense fortune, he was mean enough to retire to Woolwich, where he built a house, having one very large room, on purpose to practise as an ignorant trading justice, extorting the shillings for oaths, and the paltry fines for the harmless offences of the miserable poor around the parish. After thus continuing for a number of years the meanest and dirtiest practices of the worst of his profession, till his abuses of office had rendered the place too hot for his longer residence, he disposed of his property at Woolwich, and removed to carry on his operations at Tottenham, where he died, as above mentioned.

isinglass, relying too much on the authority of some chemical authors, whose veracity he had experienced in many other instances, he found himself constantly disappointed. Glue, not isinglass, was the result of every process; and though in the same view, a journey to Russia proved fruitless, yet a steady perseverance in the research proved not only successful as to this object, but, in the pursuit to discover a resinous matter plentifully procurable in the \* British fisheries, which has been found, by ample experience, to answer similar purposes. It is now no longer a secret that our † lakes and rivers in North America are stocked with immense quantities of fish, said to be the same species with those in Muscovy, and yielding the finest isinglass, the fisheries of which, under due encouragement, would doubtless supply all Europe with this valuable article.

No artificial heat is necessary to the production of isinglass, neither is the matter dissolved for this purpose; for as the continuity of its fibres would be destroyed by solution, the mass would become brittle in drying, and snap short asunder, which is always the case with glue, but never with isinglass. The latter indeed may be resolved into glue with boiling water, but its fibrous recombination would be found impracticable afterwards, and a fibrous texture is one of the most distinguishing characteristics of genuine isinglass. The reproduction of leather might with equal reason be attempted from the former.

A due consideration that an imperfect solution of isinglass, by the brewers called fining, possessed a peculiar property of clarifying malt liquors, induced him to attempt its analysis in cold subacid menstruums. One ounce and a half of good isinglass, steeped a few days in one gallon of stale beer, was converted into good fining, of a remarkable thick consistence: the same quantity of glue, under similar treatment, yielded only a mucilaginous liquor, resembling diluted gum-water, which, instead of clarifying beer, increased both its tenacity and turbidness, and communicated other properties in no respect corresponding with those of genuine fining. On mixing 3 spoonfuls with a gallon of malt liquor, in a tall cylindrical glass, a vast number of curdly masses became presently formed, by the reciprocal attraction of the particles of isinglass and the feculencies of the beer; which, increasing in magnitude and specific gravity, arranged themselves accordingly, and fell in a combined state to the bottom, through the well-known laws of gravitation; for, in this case, there is no elective attraction, as some

\* Upwards of 40 tons of British isinglass have been manufactured and consumed since this discovery was first made.—Orig.

† As the lakes of North America lie nearly in the same latitude with the Caspian sea, particularly Lake Superior, which is said to be of greater extent, it was conjectured they might abound with the same sorts of fish, and, in consequence of public advertisements distributed in various parts of North America, offering premiums for the sounds of sturgeon, and other fish, for the purpose of making isinglass, several specimens of fine isinglass, the produce of fish taken in these parts, have been lately sent to England, with proper attestations as to the unlimited quantity which may be procured.—Orig.

have imagined, which bears the least affinity with what frequently occurs in chemical decompositions.

These phenomena are adduced here as correlative proofs of the impracticability of making isinglass by the previous reduction of the sinewy parts of fish into jelly; and it seems evident, that the clarifying action of isinglass depends principally on a crude minute division, not solution of its parts, which is still further confirmed, by diluting a few drops of fining with fair water in a glass; for thus the slender filaments become conspicuous to the eye, especially when assisted with a double convex lens, but these immediately disappear on an addition of hot water. As the general processes for making isinglass appear from hence illusive and erroneous, the long concealed principles of its manufacture, into the various common forms and shapes, become more obvious and comprehensive. If what is commercially termed long or short stapled isinglass be steeped a few hours in fair cold water, the entwisted membranes will expand, and re-assume their original beautiful hue,\* and by a dextrous address may be perfectly unfolded. By this simple operation, we find that isinglass is actually nothing more than certain membranous parts of fishes, divested of their native mucosity, rolled and twisted into the forms above mentioned, and dried in the open air.

The sounds, or air-bladders of fresh-water fish, in general, are preferred for this purpose, being the most transparent, flexible, delicate substances. These constitute the finest sorts of isinglass; those called book and ordinary staple, are made of the intestines, and probably the peritonæum, of the fish. The Beluga yields the greatest quantity, being the largest and most plentiful fish in the Muscovy rivers; but the sounds of all fresh-water fish yield, more or less, fine isinglass, particularly the smaller sorts, found in prodigious quantities in the Caspian sea, and several hundred miles beyond Astracan, in the Wolga, Yaik, Don, and even as far as Siberia, where it is called *kla* or *kla* by the natives, which implies a glutinous matter; it is the basis of the Russian glue, which is preferred to all other kinds for its strength.

The anatomy and uses † of the sound in fish seems not yet adjusted by ichthyologists. Dossie, in his *Memoirs of Agriculture*, will have it to be the mesentery of the fish; but the celebrated Gouan, the latest, and perhaps the most accurate author on ichthyology, gives a more satisfactory and comprehensive account of it, under the title of *La Vesicule Aërienne*. Yet, if the identity of the air-bladder,

\* If the fine transparent isinglass be held in certain positions to the light, it frequently exhibits beautiful prismatic colours.—Orig.

† Fishermen have a dextrous art in perforating the sound of fresh-taken cod fish with a needle, in order to disengage the inclosed air. Without this operation, the fish could not be kept under water in the well-boat, consequently could not live; but if by accident the operator wounds an artery, the fish presently dies, through the discharge of blood, to the loss of the proprietor, who thus can seldom bring it sweet to market.—Orig.

and what in English is called sound, be admitted, which seems particularly ascertained in a certain genus, viz. the asellus of Willugby, or gadus of Artedi, his description is a little erroneous with respect to its termination near the vesica urinaria; for in cod and ling, the continuation of the sound, or air-bladder, may be easily traced from thence to the last vertebra adjoining the tail. The sounds which yield the finer isinglass, consist of parallel fibres, and are easily rent longitudinally; but the ordinary sorts are found composed of double membranes, whose fibres cross each other obliquely, resembling the coats of a bladder; hence the former are more readily pervaded and divided with subacid liquors; but the latter, though a peculiar kind of interwoven texture, are with great difficulty torn asunder, and long resist the power of the same menstruum; yet, when duly resolved, are found to act with equal energy in clarifying liquors.

Isinglass receives its different shapes in the following manner. The parts of which it is composed, particularly the sounds, are taken from the fish while sweet and fresh, slit open, washed from their slimy sordes, divested of every thin membrane which envelops the sound, and then exposed to stiffen a little in the air. In this state, they are formed into rolls about the thickness of a finger, and in length according to the intended size of the staple: a thin membrane is generally selected for the centre of the roll, round which the rest are folded alternately, and about half an inch of each extremity of the roll is turned inwards. The due dimensions being thus obtained, the two ends of what is called short staple are pinned together with a small wooden peg; the middle of the roll is then pressed a little downwards, which gives it the resemblance of a heart shape, and thus it is laid on boards, or hung up in the air to dry. The sounds which compose the long staple, are larger than the former; but the operator lengthens this sort at pleasure, by interfolding the ends of one or more pieces of the sound with each other. The extremities are fastened with a peg, like the former; but the middle part of the roll is bent more considerably downwards; and in order to preserve the shape of the three obtuse angles thus formed, a piece of round stick, about a quarter of an inch diameter, is fastened in each angle with small wooden pegs, in the same manner as the ends. In this state it is permitted to dry long enough to retain its form, when the pegs and sticks are taken out, and the drying completed; lastly, the pieces of isinglass are colligated in rows, by running packthread through the peg holes, for convenience of package and exportation.

The membrane of the book sort, being thick and refractory, will not admit a similar formation with the preceding; the pieces therefore, after their sides are folded inwardly, are bent in the centre, in such manner, that the opposite sides resemble the cover of a book; whence its name; a peg being run across the middle, fastens the sides together, and thus it is dried like the former. This

sort is interleaved, and the pegs run across the ends, the better to prevent its unfolding.

That called cake isinglass is formed of the bits and fragments of the staple sorts, put into a flat metalline pan, with a very little water, and heated just enough to make the parts cohere like a pancake, when it is dried; but frequently it is over-heated, and such pieces, as before observed, are useless in the business of fining. Experience has taught the consumers to reject them.

Isinglass is best made in the summer, as frost gives it a disagreeable colour, deprives it of weight, and impairs its gelatinous principles; its fashionable forms are unnecessary, and frequently injurious to its native qualities. It is common to find oily putrid matter and exuviae of insects between the implicated membranes, which, through the inattention of the cellar-man, often contaminate wines and malt liquors in the act of clarification. These peculiar shapes might, probably, be introduced originally with a view to conceal and disguise the real substance of isinglass, and preserve the monopoly; but, as the mask is now taken off, it cannot be doubted to answer every purpose more effectually in its native state, without any subsequent manufacture whatever, especially to the principal consumers, who hence will be enabled to procure sufficient supply from the British colonies. Until this laudable end can be fully accomplished, and as a species of isinglass, more easily producible from the marine fisheries, may probably be more immediately encouraged, it may be manufactured as follows.

The sounds of cod and ling bear great analogy to those of the acipenser genus of Linnæus and Artedi, and are in general so well known, as to require no particular description. The Newfoundland and Iceland fishermen split open the fish as soon as taken, and throw the back bones, with the sounds annexed, in a heap; but previous to incipient putrefaction, the sounds are cut out, washed from their slimes, and salted for use. In cutting out the sounds, the intercostal parts are left behind, which are much the best; the Iceland fishermen are so sensible of this, that they beat the bone upon a block with a thick stick, till the pockets, as they term them, come out easily, and thus preserve the sound entire. If the sounds have been cured with salt, that must be dissolved by steeping them in water, before they are prepared for isinglass; the fresh sound must then be laid upon a block of wood, whose surface is a little elliptical, to the end of which a small hair brush is nailed, and with a saw-knife, the membranes on each side of the sound must be scraped off. The knife is rubbed on the brush occasionally, to clear its teeth; the pockets are cut open with scissars, and perfectly cleared of the mucous matter with a coarse cloth: the sounds are afterwards washed a few minutes in lime-water, in order to absorb their oily principle, and lastly in clear water. They are then laid upon nets, to dry in the air; but, if intended to resemble foreign isinglass, the sounds of cod will only admit of that called

book, but those of ling both shapes. The thicker the sounds are, the better the isinglass, colour excepted; but that is immaterial to the brewer, who is its chief consumer.

This isinglass resolves into fining, like the other sorts, in subacid liquors, as stale beer, cyder, old hock, &c. and in equal quantities produces similar effects on turbid liquors, except that it falls speedier and closer to the bottom of the vessel, as may be demonstrated in tall cylindrical glasses; but foreign isinglass retains the consistency of fining preferably in warm weather, owing to the greater tenacity of its native mucilage. Vegetable acids are, in every respect, best adapted to fining: the mineral acids are too corrosive, and even insalubrious in common beverage.

It is remarkable that, during the conversion of isinglass into fining, the acidity of the menstruum seems greatly diminished, at least to taste, probably not on account of any alkaline property in the isinglass, but by its enveloping the acid particles. It is likewise reducible into jelly with alkaline liquors, which indeed are solvents of all animal matters; even cold lime-water dissolves it into a pulposus magma. Notwithstanding this is inadmissible as fining, on account of the menstruum, it produces an admirable effect in other respects: for, on commixture with compositions of plaster, lime, &c. for ornamenting walls exposed to vicissitudes of weather, it adds firmness and permanency to the cement; and if common brick mortar be worked up with this jelly, it soon becomes almost as hard as the brick itself: but for this purpose, it is more commodiously prepared, by dissolving it in cold water, acidulated with vitriolic acid; in which case, the acid quits the jelly, and forms with the lime a selenitic mass, while, at the same time, the jelly being deprived, in some measure, of its moisture, through the formation of an indissoluble concrete among its parts, soon dries, and hardens into a firm body; whence its superior strength and durability are easily comprehended.

It has long been a prevalent opinion, that sturgeon, on account of its cartilaginous nature, would yield great quantities of isinglass; but on examination, no part of this fish, except the inner coat of the sound, promised the least success. This being full of rugæ, adheres so firmly to the external membrane, which is useless, that the labour of separating them supersedes the advantage. The intestines, however, which in the larger fish extend several yards in length, being cleansed from their mucus, and dried, were found surprizingly strong and elastic, resembling cords made with the intestines of other animals, commonly called cat-gut, and from some trials, promised superior advantages, when applied to mechanic operations.



II. *On the Cavern of Dunmore Park, near Kilkenny, in Ireland. By Mr. Adam Walker.* p. 16.

This cavern is situated in a fine plain, rising indeed here and there into small hills. The country all round abounds with limestone, and quarries of beautiful black marble, variegated with white shells. Different from those of Derbyshire and Mendip, this cave descends perpendicularly 30 yards, from the top of a small hill, through an opening 40 yards in diameter. The sides of this pit are limestone-rock, whose chinks nourish various shrubs and trees, down which the inspector must descend with great caution. In this descent, he is amused with flights of wild pigeons, and jackdaws from the cave below. When he reaches the bottom, he sees one side of this pit supported by a natural arch of rock, above 25 yards wide, under which he goes horizontally, and sees two subterraneous openings to the right and left. If he turns to the right, he makes his way over rocks and stones, coated with spar in the most whimsical shapes, and formed from the dropping roof, just as the dripping of a candle would cover a pebble. These knobs take a fine polish, are transparent, and variegated with the wildest assemblage of colouring. The Earl of Wandesford had one of them sawn into a slab, and it is as beautiful as a moco. When these petrefactions are tried with an acid, the effervescence is excessively strong; and as the earth all round is calcarious, and the stones limestone, probably the icicle figures depending from the roof, and these knobs, are thus formed. The rains that fall on the hill over this cavern, oozing through an okery calcarious earth, and the limestone roof, imbibe or dissolve their fine particles in their descent; and as this mixture can only filter through the rock exceedingly slow, the water hanging on the roof is soon dissolved by the air, and the stony particles are left behind. Hence are formed the icicle-shaped cones that hang from the roof; these growing perpetually longer, have, in many parts of the cave, met the knobs from the bottom, and formed a number of fantastic appearances, like the pillars of a Gothic cathedral, organs, crosses, &c. When the rain filters pretty fast through the roof, it falls on the rocks below, and grows there into knobs and cones, whose vertex points to those that impend from the roof.

A spectator, viewing these, cannot but conceive himself in the mouth of a huge wild beast, with ten thousand teeth above his head, and as many under his feet. The scene is indeed both pleasing and awful; the candles burning dim, from the moisture in the air, just served to show a spangled roof perpetually varnished with water, in some places upwards of 20 yards high; in other places they crawled on all-four, through cells that will admit only one at a time. After having scrambled about 500 yards into this right hand part of the cave, they returned to day light, and then proceeded to view the left hand part. Here

were many different branches of the cavern; they tied one ball of packthread to another, as they went forward, that they might more easily find their way back. This branch is not so horizontal as the other; it declines downwards, and the openings in it are vastly wider, some being at least 100 yards wide, and above 50 high. A small rill accompanied them, which, by its different falls, formed a sort of rude harmony, well suited to the place. In a standing part of this brook, and near a quarter of a mile from the entrance, they found the bones of a hundred at least of the human race; some were very large, but when taken out of the water they crumbled away. As they could find nothing like an inscription, or earth for a burying place, they conjectured that some of the civil wars, perhaps that of 1641, might have driven the owners of these bones into this place. The tradition of the neighbourhood threw no light upon it.

Many of the rocks, on the roof and sides of this cavern, are black marble, full of white spots of a shell-like figure; and the whole neighbourhood is full of quarries of this beautiful stone, which takes a fine polish, and is used through the three kingdoms for slabs, chimney pieces, &c. In some deep and wet parts of these quarries, this elegant fossil is seen in the first stages of its formation; the shells are real, but so softened by time and their moist situation, as to be susceptible of receiving the stony particles into their pores, by whose cohesive quality, they in time become those hard white curls that give value to the marble: and it is very remarkable, and a proof that these white spots have been real shells, and thus formed, that the longer a chimney piece or slab is used, the more of those spots ripen into view.

*III. On some Specimens of Native Lead found in a Mine of Monmouthshire. By Michael Morris, M.D., F.R.S. p. 20.*

About the middle of July 1772, Dr. M. received 3 specimens of lead ore from Valentine Morris, Esq. of Piercefield in Monmouthshire. They were dug up in one of his fields, on making some drains, at no considerable depth; they were marked N<sup>o</sup> 1, 2, 3. On reducing to powder an ounce and a half of the ore, marked N<sup>o</sup> 3, in order to assay it, he perceived that several small bits were flatted by the pestle, which, on a further examination, proved to be native lead. Though the bits of lead are inconsiderable, yet, as they are the first that have been publicly seen in England, or perhaps in Europe, some of the best and latest writers on mineralogy declaring that they have not met with any, he thought it his duty to acquaint the R. S. with the fact, that the first account of native lead may appear in the Phil. Trans., as well as the first account of native tin.

*IV. Further Remarks on a Denarius of the Veturian Family, with an Etruscan Inscription on the Reverse, formerly considered. By the Rev. John Swinton, B.D., F.R.S. p. 22.*

Some years before, Mr. S. offered his thoughts on an inedited Samnite denarius,\* with some Samnite Etruscan letters, as he then apprehended, on the reverse. But as the last two letters were ill preserved, or rather in part defaced, he was not entirely satisfied with his reading of the inscription to which they appertained. He has however since met with the same coin, finely preserved, in the valuable cabinet of the Rev. Dr. Milles, Dean of Exeter, with 3 letters, in the place of the two supposititious ones, on it, perfectly formed: by the assistance of which, he has been enabled to give the true reading of the inscription, and to arrive, he flatters himself, at a full and complete interpretation of it. He is now fully convinced, from the Samnite, or Samnite-Etruscan, inscription, formerly visible on the reverse of the dean's denarius, that the true legend exhibited by his coin is NI. LVFII, or LVVII, MER, equivalent to NI. LVFIVS, or LVVIVS, MERRISS, MERRIX, or MEDDIX; who seems not to have been one of the Italian generals in the social war, as he formerly supposed, but one of the chief magistrates, either of the Oscans or the Samnites, coëval with that war; there having been 2 such magistrates, answering to the 2 Roman consuls, and the 2 Carthaginian Suffetes, in both those nations.

*V. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1771, pursuant to the Direction of the late Sir Hans Sloane, Bart., &c. p. 30.*

This is the 50th presentation of this kind, and completes the catalogue to the number of 2500 different plants.

*VI. Extract of a Letter from Mr. Ebenezer Kinnersley to Ben. Franklin, LL.D., F.R.S., on some Electrical Experiments made with Charcoal. p. 38.*

The conducting quality of some sorts of charcoal is indeed very remarkable. I have found oak, beech, and maple, to conduct very well; but tried several pieces of pine coal, without finding one that would conduct at all: perhaps they were made in a fire not hot enough, or not continued in it long enough. A strong line drawn on paper with a black lead pencil, will conduct an electrical shock pretty readily; but this perhaps may not be new to you.

On July the 12th, 1770, three houses in this city, and a sloop at one of the wharfs, were, in less than an hour's time, all struck with lightning. The sloop, with two of the houses, were considerably damaged; the other was the dwelling-

\* Vol. xii. p. 562 of these Abridgments.

house of Mr. Joseph Moulde, in Lombard-street, which was provided with a round iron conductor, half an inch thick, its several lengths screwed together, so as to make very good joints, and the lower end 5 or 6 feet under ground; the lightning leaving every thing else, pursued its way through that, melted off 6 inches and a half of the slenderest part of a brass wire fixed on the top, and did no further damage within doors, or without.

*VII. Of an Experiment made with a Thermometer, whose Bulb was painted Black, and exposed to the Direct Rays of the Sun. By Richard Watson,\* D. D., F. R. S. p. 40.*

During the hot weather, in the latter end of June and the beginning of July last (1772), Dr. W. made the following experiments at Cambridge. He exposed the bulb of an excellent thermometer to the direct rays of the sun, when the sky was perfectly free from clouds: the mercury rose to 108° of Fahrenheit's scale, and continued stationary. A fancy struck him, to give the bulb a black covering; this was easily effected by a camel's hair pencil and Indian ink; the mercury sunk a few degrees during the application of the coating, and the evaporation of the water; but presently after rose to 118°, or 10° in consequence of the black coat with which he had covered that part of the bulb which was exposed to the sun. If the bulbs of several corresponding thermometers were painted of different colours, and exposed at the same time to the sun, for a given period, some conjectures, respecting the disposition of the several primary colours for receiving and retaining heat, might be formed, which could not fail of being in some degree interesting.

*VIII. A Report of the Committee appointed by the R. S., to Consider of a Method for Securing the Powder Magazines at Purfleet. p. 42. Dated Aug. 21, 1772.*

The society being consulted by the Board of Ordnance, on the propriety of fixing conductors for securing the powder magazines at Purfleet from lightning, and having done us the honour of appointing us a committee, to consider the same, and report our opinion; we have accordingly visited those buildings, and examined, with care and attention, their situation, construction, and circumstances, which we find as follows:

They are 5 in number, each about 160 feet long, and about 52 feet wide, built of brick, arched under the roof, which in one of them is slated, with a coping of lead 22 inches wide on the ridge from end to end; and the others, as we were informed, are soon to be covered in the same manner. They stand parallel to each other at about 57 feet distance, and are founded on a chalk rock,

\* The present Bishop of Llandaff.

about 100 feet from the river, which rises in high tides within a few inches of the level of the ground, its brackish water also soaking through to the wells that are dug near to the buildings.

The barrels of powder, when the magazines are full, lie piled on each other up to the spring of the arches; and there are 4 copper hoops on each barrel, which, with a number of perpendicular iron bars, (that came down through the arches, to support a long grooved piece of timber, wherein the crane was usually moved and guided to any part where it was wanted) formed broken conductors within the building, the more dangerous from their being incomplete, as the explosion from hoop to hoop, in the passage of lightning drawn down through the bars among the barrels, might easily happen to fire the powder contained in them. But the workmen were removing all those iron bars (by the advice of some members of this society, who had been previously consulted); a measure we very much approve of.

On an elevated ground, nearly equal in height with the tops of the magazines, and 150 yards from them, is the house where the board usually meet. It is a lofty building, with a pointed hip roof, the copings of lead down to the gutters, from which leaden pipes descend at each end of the building into the water of wells of 40 feet deep, for the purpose of conveying water forced up by engines to a cistern in the roof. There is also a proof-house, adjoining to the end of one of the magazines, and a clock-house, at the distance of        feet from them, which has a weathercock on an iron spindle, and probably some incomplete conductors within, such as the wire usually extending up from a clock to its hammer, the clock, pendulum rod, &c.

The blowing up of a magazine of gunpowder by lightning, within a few years past, at Brescia in Italy, which demolished a considerable part of the town, with the loss of many lives, does, in our opinion, strongly urge the propriety of guarding such magazines from that kind of danger; and since it is now well known, from many observations, that metals have the property of conducting lightning, and a method has been discovered of using that property for the security of buildings, by so disposing and fixing iron rods, as to receive, and convey away, such lightning as might otherwise have damaged them; which method has been practised near 20 years in many places, and attended with success, in all the instances that have come to our knowledge, we cannot therefore but think it advisable to provide conductors of that kind, for the magazines in question.

In common cases, it has been judged sufficient, if the lower part of the conductor were sunk 3 or 4 feet into the ground, till it came to moist earth; but this being a case of the greatest importance, we are of opinion that greater precaution should be taken. Therefore we should advise, that at each end of

each magazine, a well should be dug in or through the chalk, so deep as to have in it at least 4 feet of standing water. From the bottom of this water should arise a piece of leaden pipe, to or near the surface of the ground, where it should be strongly joined to the end of an upright iron bar, an inch and half diameter, fastened to the wall by leaden straps, and extending 10 feet above the ridge of the building, tapering from the ridge upwards to a sharp point, the upper 12 inches of copper, the iron to be painted. We mention lead for the underground part of the conductor, as less liable to rust in water and moist places; in the form of a pipe, as giving greater stiffness for the substance; and iron for the part above-ground, as stronger, and less likely to be cut away. The pieces, of which the bar may be composed, should be screwed strongly into each other, by a close joint, with a thin plate of lead between the shoulders, to make the joining or continuation of the metal more perfect. Each rod, in passing above the ridge, should be strongly and closely connected by iron or lead, or both, with the leaden coping of the roof, by which a communication of metal will be made between the 2 bars of each building, for a more free and easy conducting of the lightning into the earth.

We also advise, in consideration of the great length of the buildings, that 2 wells, of the same depth with the others, should be dug within 12 feet of the doors of the 2 outside magazines; that is to say, one of them on the north side of the north building, the other on the south side of the south building; from the bottom of which wells, similar conductors should be carried up to the eaves, there joining well with a plate of lead, extending on the roof up to the leaden coping of the ridge, the said plate of lead being of equal substance with that of the coping. We are further of opinion, that it will be right to form a communication of lead from the top of the chimney of the proof-house to the lead on its ridge, and thence to the lead on the ridge of the corridor, and thence to the iron conductor of the adjacent end of the magazine; and also to fix a conductor from the bottom of the weathercock spindle of the clock-house, down on the outside of that building, into the moist earth.

As to the board-house, we think it already well furnished with conductors, by the several leaden communications abovementioned, from the point of the roof down into the water, and that, by its height and proximity, it may be some security to the building below it; we therefore propose no other conductor for that building, and only advise erecting a pointed iron rod on the summit, similar to those before described, and communicating with those conductors.

To these directions we would add a caution, that in all future alterations or repairs of the buildings, special care be taken that the metalline communications be not cut off or removed. It remains that we express our acknowledgments to Sir Charles Frederick, Surveyor-general of the Ordnance, for the obliging

attention with which he entertained and accommodated us on the day of inquiry. Signed, H. Cavendish, William Watson, B. Franklin, J. Robertson.

*Mr. Wilson's Dissent from Part of the preceding Report.*—I dissent from the report above, in that part only which recommends that each conductor should terminate in a point. My reason for dissenting is, that such conductors are, in my opinion, less safe than those which are not pointed. Every point, as such, I consider as soliciting the lightning, and thus, not only contributing to the increase of every actual discharge, but also frequently occasioning a discharge where it might not otherwise have happened. If therefore we invite the lightning, while we are ignorant what the quantity or the effects of it may be, we may be promoting the very mischief we mean to prevent. Whereas if, instead of pointed, we make use of blunted conductors, those will as effectually answer the purpose of conveying away the lightning safely, without that tendency to increase or invite it.

My further reasons for disapproving of points, in all cases, where conductors are judged necessary, are contained in a letter addressed to the Marquis of Rockingham, and published in the *Phil. Trans.*, vol. 54. There are other reasons also, which I have to offer, for rejecting points on this particular occasion; and which were mentioned at the committee. Those I shall lay before the R. S. at another opportunity, for the benefit of the public. Aug. 21, 1772.

*IX. Observations on Lightning, and the Method of Securing Buildings from its Effects: In a Letter to Sir Charles Frederick, Surveyor-General of the Ordnance, and F. R. S., By Benj. Wilson, F. R. S., &c. Dated Dec. 8, 1772. p. 49.*

Sir,—Your station, as Surveyor-General of his Majesty's Ordnance, being such as makes the subject of this paper particularly interesting to you, I presume an apology for this address will be wholly unnecessary. On an application of the Board of Ordnance to the R. S., in July last, a committee was appointed, to consider of the properest method for securing the magazine at Purfleet from mischief by lightning: which committee reported to the council of that learned body, what they thought necessary to be done on that occasion. The council afterwards transmitted to the board a copy of that report, together with another paper written by myself, in consequence thereof. For, during the consideration of that business, some doubts having arisen in my mind, with regard to the propriety of points, which were proposed to terminate the top of each conductor; and those doubts being founded on some experiments and observations, I could not consistently subscribe to that report, nor suppress my opinion, on a subject of such importance.

Whatever may be the sentiments of others respecting those doubts, yet, being the result of my mature consideration, I thought it my duty to propose them to the committee; and further to express my dissent, in writing, to that particular part of their report: giving, at the same time, some of the principal reasons for such dissent; and referring them, for further satisfaction on this subject, to a letter which is already published in the transactions of the R. S. Agreeable to the declaration at the end of the above dissent, I shall now proceed to offer my further reasons for objecting to pointed conductors.

Experience, which is our best guide in all physical inquiries, but particularly in electrical ones, every day convinces me, that we know but little of that subtile fluid, which operates so secretly, and at the same time so powerfully, on the earth, and its atmosphere I confess that I am even now less acquainted with the principle of its action, than I thought I was 20 years ago; the smallest differences in the circumstances of our experiments, frequently causing very material differences in their results. And perhaps no one, who has not applied his mind closely to inquiries of this kind, could conceive how the pointing a piece of metal, or not, should make any material difference in the experiment. The electrician has it always in his power to convince any one of the fact, who, through inexperience, may be inclined to entertain the least scruple about it; for even from those experiments to which it was thought proper to appeal at the committee, it appeared, that the difference in the effects on this fluid, between pointed and blunted metal, is as 12 to 1.

A thunder cloud therefore, according to that reasoning, (the circumstances of it being supposed to be nearly similar to what is called the prime conductor in those experiments), if it acted at 1200 yards distance on a point, would require a blunted end to be brought within the distance of 100 yards; and beyond those limits, would pass over it, without affecting it at all. On this occasion permit me to observe, that the longer the conductors are above any building, the more danger is to be apprehended from them; as they will in that case approximate nearer in their effects to those that are pointed. And that is one reason why I was not for advising the proposed conductors at Purfleet, to be so high as 10 feet above the magazines, and more particularly on that building called the board-house, which stands considerably higher than the magazines themselves.

But, before we advance further into this subject, it may be proper to show the reasons for introducing a pointed apparatus, when the experiment on lightning was first proposed: what good consequences were derived from that experiment; and why, on further experiments and observations, such points ought now to be laid aside, when our intention is not to make electrical experiments, but by the means of conductors, to preserve buildings from the dangerous effects of lightning.

Dr. Franklin, in his conjectures, that lightning and electricity were one and the same fluid, considered how he should invite, or bring down and collect the lightning, so as to make experiments on it. And he concluded, from observation, that the likeliest method would be, to make use of such an apparatus for the purpose, as was most susceptible of electric effects; or, in other words, such an apparatus as would receive the electric fluid with the greatest ease. Repeated experiments taught him, that metals had the property of receiving that fluid, with more ease than other substances. He also learnt, from the like experience,



that metals, by being pointed, were rendered still more susceptible of receiving it. And therefore he proposed an experiment to be tried, "Whether it was not in our power to invite, or bring down the lightning, by an apparatus, consisting of an electric stand, and an iron rod, 20 or 30 feet in length, rising upright from the middle of the stand, and at the top, terminating in a very sharp point." This apparatus was recommended to be put on some high building, with the expectation, that if a thunder cloud should happen to pass near this apparatus, some quantity of the lightning deposited therein would probably be collected in the rod, by means of the very sharp point, and the electrical stand at the foot of the rod.

That this contrivance answered the end he first proposed, we have had sufficient evidence. And it is no wonder if, after this great discovery, we find him, and other electricians, pursuing new experiments of this kind, and raising those points higher into the air, to collect still greater quantities of that fluid which occasions lightning. Nor need we be surprized, after knowing that lightning could be brought down from the heavens by so simple an apparatus, and after experiencing its subtile effects to be similar with the electric fluid, that the Americans, and others, on Dr. Franklin's recommendation, adopted the principle of securing their buildings from its dangerous effects, by raising above their houses rods of iron, very sharply pointed, and applying wires from the ends of those rods, down the outside of their houses to the ground. But though there appeared many arguments at that time in favour of such conductors, yet experiments and observations, at last, induced Dr. Franklin to alter his opinion in respect to those wires, and to substitute in their place rods of iron: still retaining the principle of having the rods at the top sharply pointed; and many of the Americans, as well as Europeans, approved of the alteration, as appeared afterwards, from constructing their conductors accordingly.

About that time great attention was given, and many new experiments were made, in consequence of the frequent dangerous effects, which lightning was observed to produce in some valuable buildings, by rending and dashing to pieces very large stones and timbers, which were connected together by cramps and bars of iron: and at other times breaking and melting part of those rods, and sometimes exploding wires, even of a considerable thickness, like so much gunpowder. From careful observations of these extraordinary appearances, produced by violent shocks of lightning; and on making other experiments relating to a certain resisting power in, or on, all bodies, which appears to act against the attacks of lightning, as well as against the electric fluid, philosophers were enabled to assign the reason, and, it is apprehended, on a solid foundation, why conductors should be made of metal, in preference to all other materials; as the power of resisting such attacks is less in metals than in wood, stone, or marble. And that this resistance might be the more simple and uniform, it

appeared the most eligible to have the conductors made of one continued piece of metal only, and of an equal diameter throughout. But what that diameter ought to be, depended on other circumstances, some of which are taken notice of in a former paper, referred to above, which I laid before the R. S.

By this historical sketch, we see the propriety of Dr. Franklin's introducing points, and the advantage philosophy has derived from them: by ascertaining that lightning and electricity are one and the same fluid: which appears to be diffused every where, at least on this earth and in the atmosphere. But when curiosity, which I apprehend was one of the first motives for introducing points to invite the lightning, was satisfied; and experience had taught us, that we had it in our power to collect that fluid which occasions it: and when the principle of its action was from experiments thus investigated and ascertained, this matter of invitation, viz. by using points, ought, in my opinion, to have ceased;\* because a greater quantity of lightning, than we have yet experienced, may chance to attack us. For we are so far from knowing how great the magazine of lightning may be in the heavens, or in the earth, when it is ready to discharge itself, either by one or more explosions, that we are ignorant even of the quantity actually discharged, whenever any stroke of lightning visits us. Nor can the ablest philosopher fix the limits of the greatest discharge that may possibly happen.

Seeing then how vain it is to look for any thing like absolute security, in all cases, it surely behoves us to proceed with caution. And it is for that reason I have always considered pointed conductors as being unsafe, by their great readiness to collect the lightning in too powerful a manner. And lest the conductors, without such points, should be too slender for very violent attacks, in places of great consequence, I have always recommended the having them above 4 times larger in diameter, than what are commonly made use of, that our security may be the greater, by opening a larger passage for any extraordinary discharge, and so far lessening the danger to be apprehended from it.

I ought not, in this place, to omit taking notice of a paper, containing some further experiments and observations, which were produced at the committee, to show, among other things, that pointed metals were more disposed to receive the lightning, by virtue of a repelling principle, in the lightning as well as the electric fluid, which acted on the natural quantity of the fluid contained within the metal, at a considerable distance from the point, causing, if I may be allowed the expression, a kind of vacuum therein; but I suppose the author means to a certain distance only.

So far from disputing this philosophy, I readily admit the fact. But, I am afraid, every attempt to prove that pointed conductors may be so disposed to

\* Unless where the electrician, like Professor Richmann (who was killed by it) at his own hazard, chuses to make further observations on lightning.—Orig.

receive this fluid more readily, will not mend the argument in the least; because, the more we lessen the power of resisting, even supposing the whole conductor to be in that state, the more we increase the power of invitation.

In regard to other experiments, with locks of cotton,\* which are acted on in a particular manner by the apposition of points, and the conclusions drawn from thence, in favour of pointed conductors, as causing similar effects on the fragments or small clouds, which, hanging below the thunder clouds, have been supposed a kind of stepping stones, for the lightning to pass on, towards the earth: such pointed conductors being supposed to occasion those fragments to retire up into the cloud whence they were suspended; and on that account, to prevent a stroke from lightning, which might otherwise have happened, I shall, for the present, wave entering in this philosophy, as I could wish the conjecture to be reconsidered: because I apprehend it is liable to many objections, which to enumerate would carry me beyond the proper bounds of such a paper as this. However, if the same opinion should again be offered, and brought in argument, it may be worth while to enter more deeply into the inquiry.

If those gentlemen, who argued at the committee for the necessity of points, could have made it appear, that such points draw off, and conduct away, the lightning imperceptibly and by degrees, without causing any explosion, during a thunder storm (which seems to have been once the opinion of Dr. Franklin) I should readily have subscribed to their report. But experience shows us, that the fact is otherwise: there being many instances, where violent explosions of lightning have happened to conductors that were sharply pointed. And 3 in particular, the accounts of which are inserted in a publication of Dr. Franklin's,† where the points were dissipated, or destroyed; and a small part of an iron rod melted next the points of one of them; and also at the several crooked ends of the rods below, where they were hooked on to each other, and formed the conductor belonging to Mr. Maine in North America. But as those letters are long, and contain several other curious facts, I shall reserve them, together with some further observations on the nature and power of that resisting principle, which is found to act so sensibly against the attacks of the electric fluid, or lightning, to some future dissertation.

There is no building, that I know of, more exposed to this kind of danger, than the Eddystone lighthouse, as it stands upon a rock in the sea, several miles from land. The fixing of a conductor to that building, was thought highly proper; and the fixing of a point on it, as highly improper. It was therefore resolved on to put up a conductor without a point, that no more lightning might be unnecessarily solicited to the building, and that all the lightning, which acci-

\* Dr. Franklin's experiments.—Orig.

† Dr. Franklin's Experiments, p. 394, 416, 417, &c.—Orig.

dentially fell on it, might be conveyed away without injuring it. This conductor was fixed 12 years ago, and the building has since received no injury from lightning.\*

There is another edifice of great consequence, I mean St. Paul's church, which stands much exposed, from its height, to accidents by lightning. The dean and chapter of that cathedral thought it an object deserving the serious attention of the Royal Society. A committee was therefore appointed, in consequence of their application: and proper conductors were put up, in the several places where they were thought necessary, from the top of the lantern to the sewers underground. And notwithstanding particular care was taken, to have the additional metal either of a considerable diameter, or an equal quantity of it formed into other shapes, for the conveniency of the several places; yet part of those conductors, consisting of iron, in the stone gallery, showed marks of their having been made considerably hot, if not absolutely red, by a stroke of lightning which happened in March last (as appears by a letter which I communicated to the R. S. from one of the vergers of that church, Mr. Richard Gould) who had examined the conductors the morning following, along with Mr. Burton of the same cathedral,† and that the appearances were in general as the verger's

\* A former building erected for the same purpose, upon this rock, was set on fire by lightning.—Orig.

† Mr. Gould acquaints us in his letter, that he examined the four conductors in the lantern and stone gallery of St. Paul's church, the morning after the lightning happened. That no marks whatever appeared on the conductor to the south, which was the first he attended to. That he next examined the conductor to the west, and observed a thick rust lying on the pavement in the stone gallery, as if it had been cleaned off, from the conductor, with a tool: that several parts of the iron appeared black, particularly the screws or nuts: something like the effects left by gunpowder on iron or steel, or a smoky fire. That the conductors to the north shewed no marks, no more than that to the south. But that on examining the conductor to the east, he found stronger marks abundantly, than on the west conductor, it being much blacker; particularly on the nut and screws: the rust lying in great quantities on the pavement. And the extreme part of the conductor, that goes into the water trunk, seemed like a piece of iron newly taken out of a forge by a smith, without working it on the anvil.

Mr. Gould has since added to the account in his letter, some circumstances which I apprehend ought not to be omitted. He says, that where the end of the conductor, on the east side, points towards the water trunk, a stone surrounds part of it, leaving an interval, half an inch wide or more, between them, and about 4 or 5 inches long, which is a little more than the breadth of the conductor. That this interval was filled up with dirt, and had been so for some time, occasioned by frequent showers of rain washing the pavement in the stone gallery. That, after the lightning happened, he observed a hole was made through the dirt, one quarter of an inch in diameter, and about 2 inches in length. That the hole was close to the iron; and that, on stooping down his head, he perceived a very disagreeable smell of sulphur from the stone, dirt, and conductor, particularly the last.

On hearing this account, Mr. Delaval and myself, a few days ago, went and examined the conductors again; but more carefully than before. For, on causing the stone to be removed, which covered the top of the water trunk, we had an opportunity of examining near 2 feet more of the iron which points to the water trunk, than we could perceive before this stone was removed. When we

letter related them to me. Mr. Delaval and myself attended, about a week afterwards, to observe them, and their particular situations, with the circumstances attending them; when we were very well satisfied with his account, notwithstanding it had rained in the interim for 3 days together.

It is worthy of note, that those conductors did not terminate in a point, nor was any point put upon the cross at the top. And yet Dr. Franklin was of that committee. If points are so essential to our safety, why was not the reason enforced at the committee, for having them on that capital edifice? For my part, I think it was a happy circumstance, that there was no point fixed on the top of the church, to solicit a greater quantity of lightning at that moment, than what fell on the conductors, circumstanced as they were: as that quantity was great enough to heat so considerably a bar of iron, near 4 inches broad, and about half an inch thick.

This powerful effect reminds me of another instance still more extraordinary, which happened in Martinico, and is related by Captain Dibden, where a bar of iron, one inch in diameter, was by a violent shock of lightning reduced in one part of it to the thickness of a slender wire only. See *Phil. Trans.*, vol. liv. p. 251.\* Since then we are at all times ignorant of the quantity of lightning in the earth and its atmosphere; and the difference in the effects, between blunted and pointed ends, in causing a discharge in our electrical experiments, appears to be as 1 to 12; it is easy to comprehend the very great danger this noble fabric has probably escaped, by having no pointed apparatus upon it.

From the above observations, I am naturally led to consider a part of the proceedings of the committee, respecting the magazines at Purfleet; when a certain number of conductors, with tapering points at the top, were resolved on, as necessary to protect the several buildings where the powder is deposited. For it was agreed on at the same meeting, that the board-house, which is a large building for the use of the board officers, and which stands considerably higher than the magazines, as was observed above, did not require any point at the top: because it was apprehended to be perfectly secure, by reason of the copings on the roof, the gutters and pipes to carry off the water being all of lead: and further, because those pipes communicated with two wells, which always contained water.

I was not a little surprized at this last resolution, which appeared to be so observed, that the conducting iron did not touch the lead. We likewise observed, that there was a very thick coat of rust all over that part of the iron; particularly at the end next the lead, where the water entered the trunk. As the necessity of attending to these circumstances will be obvious to any one, who is but in the least degree acquainted with these researches, the danger of neglecting them will be seen in the strongest light, by the gentlemen of the committee who recommended the conductors for the security of that cathedral.—Orig.

\* Abridgment, vol. xii. p. 149.

inconsistent with the former. Because, if points were necessary in one place, they ought to be so in another. And on the other hand, if the board-house is secure by the leaden accidental conductors, which have no points, why ought not the magazines to be equally secure, when put into the same circumstances? I therefore enforced the inconsistency of such a resolution in the strongest terms. Notwithstanding which, the gentlemen at that time thought proper to confirm their resolution. However, at the next meeting of the committee, I observed that they had been pleased, in the mean time, to make an amendment in favour of points for the board-house; which amendment was no sooner proposed than approved of.

Why my observation was rejected at the preceding meeting, I must leave to the judgment of others. But it certainly carries an appearance, as if manifest contradiction, on further reflection, must have been the cause of that alteration.

And I am inclined to believe, from some gentlemen of the committee expressing their opinion, 'of its being a matter of mere indifference whether blunted or pointed conductors were made use of,' that they have not considered this subject with all the due attention, which so important an object deserves. For if our experiments show, that points, from the nature of their shape, and other circumstances attending them, resist the attacks of this fluid less than blunted ones; and that blunted conductors, of proper dimensions, are sufficient to convey away the lightning safely, whenever it attacks them; why should we have recourse to a method, which is at best uncertain; and which some time or other may be productive of the most fatal effects?

But perhaps no argument can be brought with more force against the principle of points, than Dr. Franklin's own words, which are published in his experiments, p. 481, where he declares positively, 'buildings, that have their roofs covered with lead, or other metal, and spouts of metal continued from the roof into the ground to carry off the water, are never hurt by lightning; as whenever it falls on such a building, it passes in the metals, and not in the walls.'

This is the case with the British Museum, a building also of considerable consequence, where there are no other conductors, than what are formed by the copings, gutters and pipes, which are all of lead, and communicate with the ground. Now it is from the great quantity of metal contained in the several pipes, together with the other circumstances attending them, that I considered that building, in a former paper laid before the A. S., as being sufficiently secured from those dangerous accidents. But if any gentleman should be disposed to entertain a doubt about it, or indeed of any other part of my reasoning on this subject, a declaration of those doubts may be attended with good consequences, as they will necessarily open the door to a more minute investigation.

I have now, sir, gone through the reasons which I proposed to lay before the

R. S. for the rejecting of points. And I am very sorry, in the course of this letter, to have been under the necessity of mentioning any differences in opinion, which passed between the members of the committee, to whom this important matter was referred. I think however I shall stand excused to the society, and the public, when it appears, as I hope it now sufficiently does, what my motive has been; namely, to state clearly, and impartially, the objections which I conceived to lie against pointed conductors: and to disclose without any reserve, the principles on which such objections are grounded.

P. S. Mr. Delaval, who was one of the committee, has given me leave to insert his opinion on this subject; which is this: That he concurs with me in thinking that such conductors as are elevated higher than the buildings to which they are applied, or are pointed at the top, are improper and dangerous. He was desirous of delivering his opinion at the committee: but as the meetings of it were held in the summer only, his absence from London prevented his attendance.

*X. A Letter to Sir John Pringle, Bart., Pr. R. S., on pointed Conductors. Dated Dec. 17, 1772. p. 66.*

SIR,

Having heard and considered the objections to our report, concerning the fixing pointed conductors to the magazines at Purfleet, contained in a letter from Mr. Wilson to Sir Charles Frederick, and read to the R. S., we do hereby acquaint you, that we find no reason to change our opinion, or vary from that Report. We have the honour to be, &c.

H. Cavendish, W. Watson, B. Franklin, J. Robertson.

*XI. Astronomical Observations made at Chislehurst in Kent. By the Rev. F. Wollaston, F.R.S. p. 67.*

Mr. W. the last year sent to the Society an account of the going of an astronomical clock with a wooden pendulum, for the year preceding; with such observations as he had made in this place; the latitude of which is  $51^{\circ}24'33''$  N., and its longitude  $4'39'' = 16^{\circ}.6$  in time, east of the Royal Observatory at Greenwich. The rate of the clock deduced from the observations of this last year, will not be found so uniform as the foregoing. To what cause to ascribe it, he is not certain. He thinks not to heat: perhaps to the great drought of the summer. However, its acceleration or retardation was not desultory, but sufficient to be depended on for any intermediate time. The clock was cleaned in November; and when set up again, lost, between the 18th and 28th, at the rate of  $7^{\circ}.8$  per day. The regulator was then altered, and clock set, and from that time never meddled with. Then follow several observations on the going of

the clock, for the year 1771. Next, observations on the barometer and thermometer, not necessary to be reprinted. The next observation is a solar eclipse, Oct. 25, 1772, the end observed at 20<sup>h</sup> 36<sup>m</sup> 34<sup>s</sup> apparent time. Then several occultations of stars by the moon. Then follow observations of the eclipses and occultations of Jupiter's satellites, also of his belts.

*XII. On the Early Cultivation of Botany in England; and some Particulars of John Tradescant, a Great Promoter of that Science, as well as of Natural History, in the last Century, and Gardener to King Charles I. By Dr. Ducarel, F.R.S., F.S.A. p. 79.*

The sciences we know are subject to revolutions. But is it not a very extraordinary one that botany, so useful to mankind, and so well known to the ancients, should for some ages abandon Europe, and remain almost unknown there till the 16th century; when it is supposed to have suddenly revived; and has since, by the industry of the moderns, been brought to the highest perfection? The truth however is, that botany returned into England long before this æra. It was brought back here by the Saxons; since whose time, it has always flourished, more or less, in this kingdom. Dr. D. founds his opinion on the authority of the 4 following Saxon manuscripts.

Two in the Bodleian Library, viz.

Nº 4125. Herbarium Saxonicum.

5169. Liber medicinalis ms. continens virtutes herbarum Saxonice.

And two others in the Harleian Library, viz.

Nº 5066. entitled, Herbarium Saxonice.

585. Tractatus qui ab Anglo-Saxonibus dicebatur LIBER MEDICINALIS: scil. L. Apuleii Medaurensis Libri de Virtutibus Herbarum, Versio Anglo-Saxonica.

This Lucius Apuleius of Medaura was a famous Platonic philosopher, who flourished about A. D. 200. From this time he has met with no ms. concerning botany, till the 13th century, when Bishop Tanner mentions three mss. on this subject, written by Gilebertus Legleus, sive Anglicus, a physician, who flourished in the year 1210, entitled,

1. De Virtutibus Herbarum, ms. Bodl. Digb. 75. 2. Gilberti Liber de Viribus et Medicinis Herbarum Arborum, et Specierum, ms. olim Monast. Sion. 3. De Re Herbaria, lib. i.

The bishop likewise mentions one John Arden, a famous surgeon, who lived at Newark in Nottinghamshire from 1349 to 1370, as the author of a ms. (now extant in Sir Hans Sloane's library), entitled, Volumen Miscellaneorum de Re Herbaria, Physica, et Chirurgica.

In the Ashmolean library are the following mss. viz.

(Nº 704.) entitled, A Treatise of Chirurgery, with an Herbal, &c. in Old English, 4to, 1438. And another, Nº (7709.) called, an Herbary, &c. written alphabetically, according to the Latin names, in 1443. And (Nº 7537.) entitled, A Book of Plants and Animals, delineated in their natural colours on vellum, Old English, A. D. 1504.



Mr. Ames, in his *Typographical Antiquities*, p. 470, informs us, that in the year 1516, a folio, entitled, 'The Greate Herball,' was printed in Southwark by Peter Treveris; and this Dr. D. believes, is the oldest English herbal now extant in print.

To come to later times: Mr. Gough (in his *British Topography*, p. 61) informs us, 'That, before the year 1597, John Gerrard, citizen and surgeon of London, seems to be the first who cultivated a large physic garden, which he had near his house in Holborn, where he raised 1100 different plants and trees.' He might have added, that Gerrard had another physic garden in Old-street, containing a great variety of plants; a printed catalogue of which is to be found in the libraries of the curious. But Gerrard had a famous contemporary, who greatly advanced that valuable science, and of whom but little has hitherto been said by the modern biographers.

John Tradescant is the person meant. And an attempt to revive the memory of this once eminent botanist and virtuoso may not be displeasing. John Tradescant was, according to Anthony Wood, a Fleming, or a Dutchman. We are informed by Parkinson, that he had travelled into most parts of Europe, and into Barbary; and, from some emblems remaining on his monument in Lambeth church-yard, it plainly appears that he had visited Greece, Egypt, and other eastern countries. In his travels, it is supposed he collected not only plants and seeds, but most of those curiosities of every sort, which, after his death, were sold by his son to the famous Elias Ashmole, and deposited in his Museum at Oxford.

When he first settled in this kingdom, cannot at this distance of time be ascertained; perhaps it was towards the latter end of the reign of Queen Elizabeth, or the beginning of that of King James the First. His print, engraven by Hollar before the year 1656, which represents him as a person very far advanced in years, seems to countenance this opinion. He lived in a great house at South Lambeth, where there is reason to think his museum was frequently visited by persons of rank, who became benefactors to it: among these were King Charles the 1st, to whom he was gardener. Henrietta Maria his queen, Archbishop Laud, George Duke of Buckingham, Robert and William Cecil, Earls of Salisbury, and many other persons of distinction. John Tradescant may therefore be justly considered as the earliest collector in this kingdom, of every thing that was curious in natural history, viz. minerals, birds, fishes, insects, &c. He had also a good collection of coins and medals of all sorts, besides a great variety of uncommon rarities. A catalogue of these, published by his son, contains an enumeration of the many plants, shrubs, trees, &c. growing in his garden, which was pretty extensive. Some of these plants are, if not totally extinct, at least become very uncommon, even at this time. A list of some remarkable ones

introduced by him, is inserted below.\* And this able man, by his great industry, made it manifest, in the very infancy of botany, that there is scarcely any plant in the known world, that will not, with proper care, thrive in this kingdom. When his house at South Lambeth, then called Tradescant's Ark, came into Ashmole's possession, he added a noble room to it, and adorned the chimney with his arms, impaling those of Sir William Dugdale, whose daughter was his 3d wife, where they remain to this day.

It were much to be wished, that the lovers of botany had visited this once famous garden, before, or at least in the beginning of the present century. But this seems to have been totally neglected till the year 1749, when Dr Watson and Dr. Mitchel favoured the R. S. with the only account now extant, of the remains of Tradescant's garden. In it, Dr. Watson seems to confine the extent of it to that now belonging to Mr. Small's house. Dr. D. believes it was otherwise; and, on account of the great number of plants, trees, &c. is inclined to think that Tradescant's garden extended much farther. Bounded on the west by the road, on the east by a deep ditch, still extant, it certainly extended a good way towards the north, and took in not only Dr. D.'s orchard and garden, but

\* From Parkinson's Garden of Pleasant Flowers, printed in 1656.

1. 'Pseudonarcissus aureus maximus flore pleno, sive roseus Tradescanti. The greatest double yellow bastard daffodil, or John Tradescant's great rose daffodil. This daffodil was primarily introduced by John Tradescant, and for its extreme beauty, may well be entitled the glory of daffodils.' page 102.

2. 'Moly Homericum, vel potius Theophrasti. The greatest moly of Homer, 141.

3. 'Moly Indicum, sive Caucason. Indian moly, ibid. Both the above molys are natives of Spain, Italy and Greece, and were procured from thence by John Tradescant, and flourished with him, in his garden at Canterbury.' (Should be South Lambeth).

4. 'Ephemerum Virginianum Tradescanti. John Tradescant's spider-wort of Virginia. This spider-wort is of late knowledge, and for it the Christian world is indebted unto that painful industrious searcher and lover of all nature's varieties John Tradescant.' 152.

5. 'Gladiolus Byzantinus. Corn-flag of Constantinople. With this species John Tradescant observed many acres of ground in Barbary overspread, 190.

6. 'Elleborus albus vulgaris. White hellebore. This groweth in many places in Germany, and also in some parts of Russia, and in such plenty, that John Tradescant observed quantity sufficient to load a good ship with the roots, 346.

7. 'Nardis montana tuberosa. Knobbed mountain valerian. Discovered in a botanic excursion by J. Tradescant, 388.

8. 'John Tradescant introduced a new strawberry, with very large leaves, from Brussels; but in the course of 7 years, could never see one berry completely ripe. 528.

9. John Tradescant procured a new and great variety of plums from Turkey, and other parts of the world. 575.

10. 'The Argier, or Algier apricot. This, with many other sorts, John Tradescant brought with him, returning from the Argier voyage, whither he went with the fleet that was sent against pirates, Anno 1620.' 579.

Thus far Parkinson; but whether or no these plants bear his name at this period, I can no more pretend to assert than that all the species therein mentioned are even now existing in our gardens.—Orig.

also those of two or three of his next neighbours; and some ancient mulberry trees, planted in a line towards the north, seem to confirm this conjecture.

When the death of John Tradescant happened, Dr. D. has not been able to discover, no mention being made of it in the register book of Lambeth church. A singular monument was erected in the south-east part of Lambeth churchyard, in 1662, by Hester, the relict of John Tradescant the son, for himself and the rest of this family, which is long since extinct.\* This once beautiful monument has suffered so much by the weather, that no just idea can now, on inspection, be formed of the north and south sides. But this defect is happily supplied from 2 fine drawings, preserved in Mr. Pepys's library at Cambridge. We see on the east side, Tradescant's arms; on the west, a hydra, and under it a skull; on the south, broken columns, Corinthian capitals, &c. supposed to be ruins in Greece, or some other eastern countries; on the north, a crocodile, shells, &c. and a view of some Egyptian buildings. Various figures of trees, &c. in relievo adorn the four corners of this monument.

*XIII. Of the Intense Cold in the Months of Jan. 1767, and 1768, and Nov. 1770, observed at Franeker. By J. H. Van Swinden, Prof. Philos. in the Acad. of that place, and Fellow of the Harlem Soc. From the Latin. p. 89.*

In Jan. 1767, Fahrenheit's thermometer showed degrees as follow: Jan. 6<sup>d</sup> 8<sup>h</sup> a. m. 16°; at 10<sup>h</sup> p. m. 2°.—Jan. 7<sup>d</sup> 7<sup>h</sup> a. m. — 2°; at noon + 12°; at 7<sup>h</sup> p. m. 5°; at 9<sup>h</sup> p. m. 7°.—Jan. 8<sup>d</sup> 7<sup>h</sup> a. m. 12°.—Here the lowest was — 2° or 2 below 0, the mercury being all sunk down into the bulb. But M. Vander Bild observed it as low as — 5.

In 1768, Jan. 3 it was as low as — 3<sup>+</sup>.

In 1770, the lowest in Nov. was + 9, viz. on the 20th, at 7<sup>h</sup>.

*XIV. An Inquiry into the Quantity and Direction of the Proper Motion of Arcturus; with some Remarks on the Diminution of the Obliquity of the Ecliptic. By Tho. Hornsby, M.A., Savilian Prof. of Astron. Oxford, and F.R.S. p. 93.*

By comparing ancient with the best modern observations, it appears that some of the fixed stars have a proper motion, independent of any motion hitherto known in our own system; or that, in other words, the angular distances of the fixed stars have not always continued the same, and in some of them the alteration is so very considerable, as to be easily perceived in the course of a few years, with instruments accurately made, and nicely adjusted. Of all the stars visible

\* John the grandson, buried 15th September 1652.

John the son, buried 25th April 1662.

Hester, widow of John Tradescant, buried 6th April 1678. From the register of Lambeth Church. —Orig.

in our hemisphere, the variation in the place of Arcturus is the most remarkable, and such as cannot possibly be attributed to the uncertainty of observation. It has accordingly been noticed by many astronomers: in particular, Dr. Halley mentions it in N° 355 of the *Phil. Trans.* M. Cassini, in the *Memoirs of the Academy of Sciences* for 1738, p. 231, has shown, that there is a variation of 5' in the latitude of that star, between his own time and that of Tycho, in an interval of a century and a half; and M. le Monnier, in the *Memoirs of the Academy of Sciences* for 1767, p. 417, proves, that the latitude of Arcturus varies at the rate of 2" every year; and that the longitude decreases at the rate of 60" in 100 years.\* But as an inquiry both into the true quantity and into the direction of this motion, has not hitherto been made public, Mr. H. proposes to give some account of his own observations, made expressly with this view in the years 1767 and 1768, with a transit instrument of 44 inches, and a moveable mural quadrant of 33 inches, both constructed by Mr. Bird, and of the conclusions resulting from a comparison between them and some observations made by Mr. Flamsteed in 1690.

It may perhaps be objected, that the differences of right ascension, as determined by Mr. Flamsteed's mural instrument, are not to be depended on, from the very nature of his instrument. Mr. Flamsteed was himself too good an observer not to be aware of this, and accordingly, in the *Prolegomena* to the 3d volume of the *Historia Cœlestis*, p. 132, he informs us in what manner he determined the error of the plane at different distances from the zenith. By distributing these errors in the best manner, Mr. H. is of opinion, that the error of the plane of his instrument may be supposed to decrease uniformly at the rate of half a second in time for every degree of zenith distance from 28° to 60°, the error being 39" at the former, and 23" at the latter, by which quantity stars passed the horary wire, in his instrument, before they came to the true meridian. It should seem also, that the error continued nearly the same from 60° to 75°, being at the latter only 22"; but that it decreased irregularly from 75° to 85°, viz. 1" in time for each degree from 75° to 80°, and 0".4 for each degree from 80° to 85°. The mural arc was fixed on a stone pier, the southern part of which was found to settle yearly, whence the error of the line of collimation to the south necessarily became every successive year greater and greater. As Mr. Flamsteed seems not to have had any method of adjusting his instrument by a plumb-line, these errors must have been irregular at different seasons of the same year, and were perhaps never truly determined. But as the observations here referred to were made on the same day, and within the compass of an hour,

\* See also the *Memoirs of the Academy of Sciences* for 1769, p. 21. See also *Astronomiæ Fundamenta*, by the Abbé de la Caille; who, in reducing his observations of Arcturus, supposes the annual motion of declination in that star = 19", p. 169, and 187.—Orig.

they are probably not affected with this latter error. We are at present concerned with the difference of 2 zenith distances, and not with the absolute quantity of them. The conclusions may indeed be affected with an error in the divisions; and from the examination which Mr. H. has been able to make, he is of opinion that the arc of Mr. Flamsteed's instrument was not of the proper quantity; and that, though the observations generally erred in defect, yet in some parts they erred in excess.

On Feb. 14, 1690, Mr. Flamsteed observed, that a small star, of the 7th or 8th magnitude, whose place is not determined in the British catalogue, and which star was named by him *Infra Arcturus*, preceded *Arcturus*  $3^s$  in time, or  $3^s.3$ , when an allowance is made for the error of the plane of the instrument =  $0' 42''.6$ , and was  $26' 30''$  to the south of *Arcturus*.\* By a mean of 8 observations made at Oxford, on or near June the 10th, 1767, with the transit instrument, and with a refracting telescope of 8 feet, furnished with a micrometer; the difference of right ascension was  $1' 8''.75$  of a degree, the star following *Arcturus*; and by a mean of 3 observations, the extremes differing only  $3''$ , the small star was  $23' 55''.0$  to the south of *Arcturus*.

The right ascension of *Arcturus* and the small star being nearly the same, the change in declination ought to be so likewise. But, from the observed difference in declination, the right ascension of the two stars must vary unequally, though with a very small difference. Accordingly it appears from computation (in which the annual precession is supposed =  $50''.35$ , the obliquity of the ecliptic at the middle of the interval of the time =  $23^\circ 28' 30''$ , and the right ascensions and declinations of the two stars taken at a mean between the times of observation) that the variation of *Arcturus* in right ascension was  $3270''.6$ , and of the small star  $3277''.6$ , in 77.287 years. Therefore the right ascension of *Arcturus* alters less than that of the star; and consequently *Arcturus* should in 1767 have followed the star by  $42''.6$ . But the star was observed to follow *Arcturus* by  $1' 8''.75$ . The right ascension therefore of *Arcturus* has increased less than that of the star, or *Arcturus* has moved westward  $1' 51''.35$  in 77.287 years; and has gone southward  $2' 35''$  in the same time, supposing the small star not to have moved, which is highly probable.

On the same day the difference of right ascension in time between the star  $\gamma$  Bootis and *Arcturus* was  $21^m 32^s$  of mean solar time, =  $5^\circ 24' 2''.2$ , when a proper allowance is made for the going of the clock, and for the error of the plane of the instrument, and the difference of declination was  $50' 45''.6$ , when an allowance is made for refraction. On the 24th, 26th and 29th, of May, and the 9th of June of the year 1768, Mr. H. determined the difference in

\* This is the only observation of that star made by Mr. Flamsteed.—Orig.

right ascension to be  $21^m 27^s$  of sidereal time by the two former observations, and  $21^m 26\frac{3}{4}^s$  by the two latter, the difference in declination being  $49' 48''.7$ , by a mean of the observations in May, the extremes differing only 4 seconds. It appears from computation, that between the times of observation the variation of  $\pi$  Bootis in right ascension was  $3371''.7$ , and  $1417''.3$ , in declination; of Arcturus  $3311''.7$  in right ascension, and  $1347''.9$  in declination; the difference of variation in right ascension is  $1' 0''$ , and of declination  $1' 9''.4$ ; by the former the difference in right ascension was diminished, and in declination increased by the latter, agreeably to the places of the two stars. The difference in right ascension therefore in 1768, if neither of the stars had moved, should have been  $= 5^\circ 23' 2''.2$ , and  $51' 55''$  in declination; but they were observed to be  $5^\circ 21' 43''.4$ , and  $49' 48''.7$ . Arcturus therefore, by this observation, has in 78.257 years gone  $1' 18''.8$  to the west, and  $2' 6''.3$  to the south, supposing  $\pi$  Bootis not to have any proper motion.

On the 5th of April, 1691, the difference in right ascension between  $\pi$  Bootis and Arcturus was  $21^m 33^s$  of mean solar time,  $= 5^\circ 24' 14''.0$ ; and the difference of declination  $50' 45''.6$ , as in the preceding example. The difference of variation in right ascension is  $59''.1$ , and in declination  $1' 8''.4$ . The difference of right ascension therefore at the latter end of May, 1768, should have been  $5^\circ 23' 14''.9$ , and  $51' 54''.0$  in declination; but, according to observation, they were  $5^\circ 21' 43''.4$ , and  $49' 48''.7$ . Arcturus therefore, according to this observation, has moved  $1' 31''.5$  to the west, and  $2' 5''.3$  to the south in 77.120 years.

On the 4th of May, 1691, the difference of right ascension between  $\pi$  Bootis and Arcturus was  $21^m 33^s$  of mean solar time,  $= 5^\circ 24' 14''.3$ , when allowance is made for the going of the clock and the error of the plane of the instrument, and the difference of declination on the 3d of May  $= 50' 50''.6$ . According to computation, those differences should have been  $5^\circ 23' 15''.2$  and  $51' 59''.0$  respectively; but they were observed to be  $5^\circ 21' 43''.4$  and  $49' 48''.7$ . Arcturus therefore, in 77.071 years, has moved  $1' 31''.8$  westward, and  $2' 10''.3$  southward. N. B. The zenith distance of Arcturus, as determined by Mr. Flamsteed, on the 4th of May, is manifestly erroneous.

On the 27th of May, 1692,  $\pi$  Bootis preceded Arcturus in right ascension by  $21^m 32^s.5$  of mean solar time,  $= 5^\circ 24' 10''.1$ , the difference of declination being  $50' 50''.6$ . In 75.978 years the difference of right ascension should have been  $5^\circ 23' 11''.8$ , and  $51' 58''.0$  in declination; but those differences were observed to be  $5^\circ 21' 43''.4$  and  $49' 48''.7$ . Arcturus therefore has moved  $1' 28''.4$  westward, and  $2' 9''.3$  southward.

On the 27th of May, 1692, Arcturus preceded  $\pi$  Bootis in right ascension by  $24^m 35^s.5$  of mean solar time,  $= 6^\circ 9' 32''.2$ , when an allowance is made for the going of the clock and the error of the plane of the instrument, the differ-

ence of declination being  $3^{\circ} 2' 28''.9$ . On the 24th and 26th of May, and 5th of June, 1768, the difference of right ascension between the same stars observed at Oxford was  $24^m 44^s.58$  of sidereal time,  $= 6^{\circ} 11' 9''.1$ , the difference of declination being  $2^{\circ} 58' 24''.2$ . In 1768, the difference of right ascension should have been  $2^s.7$  greater,  $= 6^{\circ} 9' 34''.9$ ; and the difference of declination  $1' 31''.7$  less,  $= 3^{\circ} 0' 57''.2$ . But they were observed to be  $6^{\circ} 11' 9''.1$ , and  $2^{\circ} 58' 24''.2$ . Arcturus therefore in 75.978 years has, by a comparison with this star, moved  $1' 34''.2$  westward, and  $2' 33''.0$  southward.

Again, the difference of declination between Arcturus and  $\pi$  Bootis was observed to be  $3^{\circ} 2' 33''.9$  on the 14th of February, 1690, when the difference of right ascension between these two stars was not observed by Mr. Flamsteed. It appears by computation, that the difference of variation in declination between the times of observation was  $1' 34''.5$ , by which quantity the difference of declination was diminished, and should therefore in 1768 have been  $3^{\circ} 0' 59''.4$ . But it was  $2^{\circ} 58' 24''.2$  by actual observation. Arcturus therefore by this observation has moved southward  $2' 35''.2$  in 78.255 years.

By the foregoing comparisons Arcturus appears to have moved as in the following table.

	Years.	Westward.	Southward.
By the small star Feb. 14, 1690, in 77.237.....		$1' 51''.35$ .....	$2' 35''.0$
$\eta$ Bootis Feb. 14, 1690, in 78.257.....		$1' 18''.8$ .....	$2' 6''.3$
$\eta$ Bootis April 5, 1691, in 77.120.....		$1' 31''.5$ .....	$2' 5''.3$
$\eta$ Bootis May 4, 1691, in 77.071.....		$1' 31''.8$ .....	$2' 10''.3$
$\eta$ Bootis May 27, 1692, in 75.978.....		$1' 28''.4$ .....	$2' 9''.3$
By $\pi$ Bootis May 27, 1692, in 75.978.....		$1' 34''.2$ .....	$2' 33''.0$
$\eta$ Bootis Feb. 14, 1690, in 78.257.....		not obs. ....	$2' 35''.2$

As the quantity of the motion of Arcturus southward in declination, as deduced from a comparison with  $\eta$  Bootis, differs considerably from the quantities given by the small star and  $\pi$  Bootis, which agree very nearly together, Mr. H. compared  $\eta$  Bootis with some of the neighbouring stars, as that star, though of the 3d magnitude only, may have a small motion of its own. On the 14th of Feb. 1690, the difference of declination between  $\eta$  and  $\pi$  Bootis was observed by Mr. Flamsteed to be  $= 2^{\circ} 11' 47''.8$ . By computation, that difference in 1768 should have been  $2^m 43^s.9$  less,  $= 2^{\circ} 9' 3''.9$ : but it was actually observed to be  $2^{\circ} 8' 34''.3$  only. The star  $\eta$  Bootis therefore appears by this comparison to have moved southward  $29''.6$  in 78.257 years.

On the 27th of May, 1692,  $\eta$  Bootis was observed by Mr. Flamsteed to be  $2^{\circ} 11' 37''.8$  to the north of  $\pi$  Bootis, which quantity should by computation be  $2' 39''.1$  less in 1768, or  $2^{\circ} 8' 58''.7$ . But it was found to be  $2^{\circ} 8' 34''.3$ . The star  $\eta$  therefore appears to have moved southward  $24''.4$  in 75.978 years.

On the 25th of April, 1693,  $\eta$  Bootis was observed to be  $40' 20''.8$  to the south of  $\epsilon$  Bootis, a star of the 6th magnitude; and by Mr. H. that difference

was observed to be  $42' 37''.5$ , by taking the mean of two observations on the 24th and 26th of May, 1768, differing only  $4''.7$ . According to computation, the variation of  $\pi$  Bootis in declination during the interval of the two observations was  $1359''.3$ , and of Bootis  $1256''.0$ ; therefore the difference of variation in declination was  $1' 43''.3$ , by which the distance of the stars was increased. The difference in declination therefore in 1768, if neither of the stars moved, should have been  $42' 4''.1$ ; but it was observed to be  $33''.4$  greater, by which quantity therefore  $\pi$  Bootis must have moved southward in 75.052 years.

By reducing all the foregoing deductions to 78 years, Arcturus appears to have moved,

	Westward.	Southward.
By the small star, Feb. 14, 1690.....	$1' 52''.380$	$2' 36''.43$
$\pi$ Bootis Feb. 14, 1690.....	$1' 18''.541$	$2' 5''.88$
$\pi$ Bootis April 5, 1691.....	$1' 32''.557$	$2' 6''.75$
$\pi$ Bootis May 4, 1691.....	$1' 32''.906$	$2' 11''.87$
$\pi$ Bootis May 27, 1692.....	$1' 30''.752$	$2' 12''.74$
By $\pi$ Bootis May 27, 1692.....	$1' 36''.707$	$2' 37''.07$
$\pi$ Bootis Feb. 14, 1690.....	not observed.....	$2' 34''.69$

But the star  $\pi$  Bootis appears also to have moved southward.

By $\pi$ Bootis Feb. 14, 1690.....	$0' 29''.503$
$\pi$ Bootis May 27, 1692.....	$0' 25''.049$
By $\pi$ Bootis April 5, 1693.....	$0' 34''.712$
By a mean.....	$0' 29''.755$

As Arcturus appears to have moved southward of  $\pi$  Bootis  $2' 9''.31$ , by taking a mean of the 4 quantities resulting from the comparisons with that star; and as  $\pi$  Bootis has also moved southward of some of the neighbouring small stars by  $29''.755$  in the same time, Arcturus on the whole has moved  $2' 39''.06$  to the south, by the comparisons with  $\pi$  Bootis only; and therefore, by taking a mean of all the results, Arcturus has altered its right ascension less than the neighbouring stars by  $1' 33''.97$  in 78 years, in which time it has also moved  $2' 36''.81$  to the south of the same stars.

In order to see how far the motion of right ascension is to be depended on, which is deduced from the above comparisons, Mr. H. selected and computed the following observations, made at Shirburn castle with a transit instrument of 5 $\frac{1}{2}$  feet, placed exactly in the plane of the meridian, and consequently more to be relied on than those made with a mural instrument. By a mean of 5 observations, made on the 7th, 12th, 23d, 24th, and 31st of May, 1741, o. s., the difference in right ascension between  $\pi$  Bootis and Arcturus was  $5^{\circ} 22' 38''.9$ , the extremes differing only  $4''.4$  of a degree. The difference in the variation of right ascension to the end of May, 1768, is  $20''.5$ , by which the ascensional difference is diminished. It should therefore have been  $5^{\circ} 22' 18''.4$ ; but it was observed to be  $5^{\circ} 21' 43''.4$ . Therefore in 27 years Arcturus has moved westward  $35''.0$ .

On the 16th and 20th of May, 1744, the difference in right ascension be-



tween  $\gamma$  Bootis and Arcturus was  $5^{\circ} 22' 30''.0$  by each of the observations, which difference should have been, supposing neither of the stars to have any proper motion,  $5^{\circ} 22' 11''.7$  in May 1768. But it was found to be  $28''.3$  less; by so much therefore had Arcturus moved westward in 24 years.

On the 24th of May, and 8th of June, 1746, the difference in right ascension between the same stars was  $5^{\circ} 22' 26''.2$ , by taking a mean of the two observations; that difference should have been  $5^{\circ} 22' 9''.5$  in 1768. But it was observed  $= 5^{\circ} 21' 43''.4$ . Arcturus therefore in 22 years has moved  $26''.1$  to the west.

Lastly, on the 16th of April, and 27th and 28th of May, 1747, the difference in right ascension between  $\gamma$  Bootis and Arcturus, by taking a mean of the 3 observations, was  $5^{\circ} 22' 25''.0$ . By computation the variation in the difference of right ascension was  $16''.0$ , by which the ascensional difference should have been diminished, and  $= 5^{\circ} 22' 9''.0$ . But by observation it was found  $= 5^{\circ} 21' 43''.4$ ; Arcturus therefore by this last observation appears to have gone  $25''.6$ , westward.

By the observations therefore at Shirburn castle,	1741..0' 35''.0..1' 41''.11
Arcturus appears to have gone westward, as in the	1744..0 28 .3..1 31 .97
annexed table; in the last column of which are	1746..0 26 .1..1 32 .59
contained the quantities resulting from the obser-	1747..0 25 .6..1 34 .90
ations of each year, reduced to 78 years.	Mean 1 35 .14

The mean of all the observations, when reduced to an interval of 78 years, is  $1' 35''.14$ , which differs only  $1''.17$  from the mean of the other comparisons.

As then the proper motions of Arcturus westward in right ascension  $= 1' 33'' 974$ , and  $2' 36''.81$  in declination southward, seem well established, the real motion of Arcturus is inclined in an angle of  $30^{\circ} 56'$  to the west of the meridian or horary circle, and to be in that direction  $3' 2''.81$  in 78 years, or at the rate of  $2''.343$  in a year. As this direction of its motion is nearly perpendicular to the plane of the ecliptic, the latitude of Arcturus must diminish yearly almost in the same proportion; and its longitude will alter less than that of other stars, though not so considerably as its right ascension. The proper motion of Arcturus then, in right ascension westward, being  $1''.205$ , and in declination  $2''.005$ , its annual precession in right ascension is  $41''.108$ , and in declination  $19''.133$ ; and the true right ascension of Arcturus, on Jan. 1, 1773, is  $211^{\circ} 19' 47''.4$ , and declination north  $20^{\circ} 22' 23''.3$ .

As none of the other principal stars have been found to have a motion so considerable as this, though many of the stars of the first magnitude, as for instance, Sirius, Procyon,  $\alpha$  Aquilæ,  $\alpha$  Orionis, as also  $\beta$  Aquilæ of inferior magnitude, do really vary their positions, and perhaps all of the first order will hereafter be found to have a proper motion, we may fairly conclude, that Arcturus is the nearest star to our system, visible in this hemisphere. If therefore the

annual parallax of the fixed stars can ever be discovered, that is, if the diameter of the annual orbit bear a sensible proportion to the distance of the nearest fixed star, it is most likely to be discovered from the observations of Arcturus. The system of the world, considered in an enlarged sense, and agreeable to the idea we may entertain of an all-powerful benevolent Creator, may be taken to occupy the whole abyss of space, and to consist of an assemblage of bodies, having different magnitudes, and emitting various degrees and modifications of light. The apparent change of situation visible from the planet which we inhabit, and which revolves round one of the great bodies constituting a part of the general system, as a centre, may be owing either to the motion of our own system in absolute space, or, if our system should be at rest, to a real motion in the stars themselves: whence the angular distances of the stars must vary in proportion to the velocity of those motions, or to the direction of those motions with respect to ourselves. I have reason, at present, says Mr. H., to believe that a small motion may be discovered in the star  $\alpha$  ceti, and perhaps in other stars that vary in degrees of brightness, which the diligence of future astronomers will discover, and perhaps in less time than at first sight might seem necessary, when we consider the several improvements which have of late been made in the methods of observing the heavenly bodies.

As the motion of Arcturus in declination, the quantity of which we have thus endeavoured to ascertain, has been often acknowledged, it is matter of wonder that some astronomers, by comparing either the altitude or zenith distance of the sun's limb with Arcturus, without previously settling the quantity of that star's motion in declination, or at least doing it indirectly, should endeavour to determine whether the obliquity of the ecliptic has remained constant, or still continues to diminish, as it should seem to have done for many centuries past, from the observations of successive astronomers. M. Cassini, and M. le Monnier, have both practised this method, and are of opinion, that the obliquity of the ecliptic has not altered; or, if it has altered, that the quantity of its alteration is not near so considerable as has been imagined by some celebrated astronomers. By observing for several days, before and after the solstice, the altitude or zenith distance of the sun's limb, and that of a star situated near the same parallel, the differences to be remarked in process of time, in the distances of the sun from that star (the motion of the star in declination being allowed for during that interval of time), will be the quantity by which the sun will have approached to or have receded from the star. If the star were absolutely a fixed point, and the observations sufficiently numerous, that, by taking a mean, the necessary and unavoidable errors in observation might either be considerably diminished; or almost annihilated, the method might be practised to great advantage. But as the star Arcturus had a proper motion, and its apparent place was continually

varying from the effect of the nutation of the earth's axis; as the limb of the sun was sometimes approaching to, and sometimes receding from, the star, by a kind of libratory motion, from the effect of the nutation; and also as the obliquity of the ecliptic itself was, in all probability, continually diminishing; from a combination, and as it were involution of these motions, no certain conclusion could be drawn, since, in the space of a few years, the apparent obliquity may be the same, and yet the mean obliquity may have diminished, or perhaps, in the space of a few years, the obliquity may appear to have increased, when it may really have become less. Whereas, by reducing the observations to their mean position, and by assigning to each known cause its proper and allowed effect, a regularity and uniformity must necessarily take place, as far at least as is consistent with the unavoidable errors in observing.

M. Cassini, in the Memoirs of the Academy of Sciences for 1767, acquaints us, that, in 1748, the apparent distance of Arcturus from the upper limb of the sun, at the time of the solstice, was the same as in 1766.

In 1748, distance of Arcturus from the sun's solstitial limb.....	3°	13'	36"	40"
Altitude of Arcturus .....	61	41	17	0
Therefore the apparent solstitial altitude .....	64	55	13	40
In 1766, distance of Arcturus from the sun's solstitial limb.....	3	19	32	0
Altitude of Arcturus .....	61	35	42	0
Therefore the apparent solstitial altitude.....	64	55	14	0

The same astronomer has, in the Memoirs for 1759, p. 325, communicated the following conclusions.

	Dist. of the star from the sun's limb.	Reduction.	Solstitial distance,
1763. June 14.....	3° 7' 29"	+ 11' 1"	3° 18' 30"
15.....	3 10 16	+ 8 13	3 18 29
25.....	3 15 40	+ 2 48	3 18 28
July 1.....	2 59 1	+ 19 22	3 18 23
2.....	2 54 55	+ 23 33	3 18 23
3.....	2 50 18	+ 28 8	3 18 26
			Mean..3 18 27

Mr. Le Monnier, in the Memoirs for 1762, p. 269, has published the annexed distances of Arcturus from the limb of the sun, reduced to the solstitial point, with a view to obtain differences in the apparent obliquity of the ecliptic; and, from the observations made with the gnomon of St. Sulpice, and communicated by Mr. Le Monnier, 1738. . 3° 10' 15"

in the same volume, it should .1740. . 3 11 5

seem that that astronomer is of 1742. . 3 11 48

opinion, that the obliquity of 1763. . 3 18 40 with the mural 5-foot quad.

the ecliptic has no other varia- .. 3 18 35 with the large mural instru.

tion than what the nutation of the earth's axis will occasion; and that therefore we must either abandon the absolute diminution of the ecliptic, or at least suppose it extremely small, since, in the space of 18 years, it has not produced a sensible alteration.

As the results of the observations only, and not the observations themselves, are communicated, Mr. H. observes, that there is a very considerable difference between the conclusions of the two astronomers for the same year 1763, and declares his suspicion, that if the apparent (for such he apprehends them to be) were reduced to the mean distances, they would probably afford a confirmation of the diminution of the ecliptic. For the following observations of the sun's zenith distance, made at Shirburn castle, near the summer solstices of the years 1743, 1746, 1748, and 1766, and of Arcturus in the years 1743, 1746, and 1766, when reduced to their mean state at the solstice, do not confirm the assertion of Mr. Cassini, but are an evident and absolute proof that the obliquity of the ecliptic has sensibly diminished during an interval of 23, and even of 18 years.

The observations of 1743 were made with a mural quadrant of 5 French feet, constructed by the late Mr. Sisson: but as the linear divisions were found to be somewhat less accurate than was expected, and as the body of the quadrant was not framed with proper strength and solidity, Mr. Bird was employed in the summer of the year 1745, by the Earl of Macclesfield, (the body of the instrument having been strengthened by screwing a large and broad plate of brass on the cross bars), to put a set of points on the limb between the 90 and 96 arches of linear divisions. By these operations the line of collimation was found to have varied, and to be  $= 6''.3$ , by which the zenith distances were given too small, by the positive divisions, from the end of 1746 to the end of June 1751, when Mr. Bird bisected the spaces between the points which he had formerly added in 1745. But after the year 1751, the error of the line of collimation was  $= 2''.6$ , as appears from observations of  $\gamma$  Persei,  $\beta$  and  $\gamma$  Draconis, by which the zenith distances are also given too small; and in that state the instrument continued to the year 1767, when a new set of wires was put into the telescope, and the line of collimation thereby altered. The error of the line of collimation from 1743 to 1745 cannot directly be ascertained, for want of zenith observations; but, from some indirect methods, it should seem that the error was as nearly as possible  $= 2''$ , to be added to the observed zenith distances.

Thus by a series of observations of the sun's zenith distances, from the 7th to the 27th of June, 1743, when corrected for his semidiameter and refraction, the medium of all the 12 days, when reduced all to the solstice, is as follows:

				A similar set of 14 days observations, from May 31, to June 3, 1746, give			
The Mean.....	28°	10'	58.2"	For the Mean.....	28°	10'	52.5"
Sun's parallax.....			—4.1	Sun's parallax.....			—4.1
	28	10	54.1		28	10	48.4
Nutation.....			+6.7	Nutation.....			+9.4
	28	11	0.8		28	10	57.8
Line of collimation.....			+2.	Error of the line of collimation			+6.3
Mean solstitial zenith dist., 1743	28	11	2.8	Mean solstitial zenith dist., 1746	28	11	4.1

A like set of 8 days observations, from the 15th to the 29th of June, 1748, give,			And again another set of 10 days observation, from the 11th to the 20th of June, 1766, give,		
For the mean	28° 10'	55.8"	For the mean	28° 11'	10.5"
Sun's parallax	28 10	—4.1	Sun's parallax	28 11	—4.1
Nutation	28 10	51.7	Nutation	28 11	6.4
	28 10	+6.1		28 11	+7.6
Error of the line of collimation		57.8	Error of the line of collimation		14.0
		+6.3			+2.6
Mean solstitial zenith dist., 1748	28 11	4.1	Mean solstitial zenith dist., 1766	28 11	16.6

Then follow a series of 10 days observations of the zenith distances of Arcturus, from May 12 to July 1, 1743; when these are corrected for refraction, aberration, nutation, precession, the medium of all is as follows: viz.

The mean	31° 7'	33.6"
Error of the line of collimation		+2
Mean zenith distance of Arcturus, June 21, 1743	31 7	35.6
Mean zenith distance of the sun's centre, June 21, 1743	28 11	2.8
Mean distance of Arcturus from the sun's centre, 1743	2 56	32.8

In like manner, 5 days observations, from June 4 till Oct. 9, 1746, give for

The mean	31° 8'	26.4"
Error of the line of collimation		+6.3
Mean zenith distance of Arcturus, June 21, 1746	31 8	32.7
Mean zenith distance of the sun's centre, June 21, 1746	28 11	4.1
Mean distance of Arcturus from the sun's centre, 1746	2 57	28.6

Again, a like set of 4 days observations, from May 13, to June 23, when corrected, give for

The mean	31° 14'	50.3"
Error of the line of collimation		+2.6
Mean zenith distance of Arcturus, June 21, 1766	31 14	52.9
Mean distance of the sun's centre, June 21, 1766	28 11	16.6
Mean distance of Arcturus from the sun's centre, 1766	3 3	30.3

From the foregoing observations, it appears that the mean solstitial zenith distance in summer was as here annexed: and by comparing the 3 former with the latter, the variation of the obliquity of the ecliptic in 100 years is as is expressed in the last column of the table.

By comparing the distance of Arcturus from the sun's centre in 1743, with the same distance as observed in 1766 (an allowance being made for the proper motion of the star during the interval, as also for its variation in declination arising from the precession of the equinoxes), it appears that its distance is 17".3 less than it would have been, if the distance of the sun's centre from the equator had remained unvaried. By that quantity therefore the obliquity of the ecliptic has altered in 23 years; which is at the rate of 75".2 in 100 years. By comparing, in like manner, the distance in 1746, the obliquity of the ecliptic has diminished 15".6 in 20 years, or 78" in 100 years.

Distance in 1743	2° 56'	32.8"	In 1746,	2° 57'	28.6"
Motion of the star in decl. southward	7	20.8		6	23.3

Variat. in 100 years.

1743..	28° 11'	2.8"	60"
1746..	28 11	4.1	62.5
1748..	28 11	4.1	69.4
1766..	28 11	16.6	

Computed distance in 1766 .....	3° 3' 53.6"	In 1746, 3° 3' 51.9"
Observed distance in 1766 .....	3 3 30.3	3 3 36.3
Variation of obliquity .....	17.3	15.6

The foregoing deductions prove, Mr. H. thinks, beyond all doubt, that the obliquity has become less; but as the interval of time between the two terms of comparison is so short, that the errors committed in observing, may bear a sensible proportion to the small quantities just now found, and which perhaps are somewhat too large; Mr. H. has recourse to Mr. Flamsteed's observations, and compares them with observations made by himself, in the course of the last and present years. For this purpose he reduced all the observations of the sun, made in 1690, from May 26 to June 24, o. s. and also all the observations of Arcturus, made in the same year, to their mean position at the summer solstice of that year. The observations, together with his own made at Oxford, are as follow: These observations of the zenith distances, both of the sun and Arcturus, when corrected as the preceding, and the medium of all taken, give first, for the sun's zenith distance at the solstice,

The mean .....	28° 0' 54.2"	Again, for the sun's zenith distance in 1771,	
Error of the line of collimation ..	— 1 30	The mean is .....	28° 17' 8.7"
	27 59 24.2	Sun's parallax .....	— 4.1
Sun's parallax .....	— 4.1		28 17 4.6
	27 59 20.1	Nutation .....	— 6.8
Nutation .....	+ 9.5		28 16 57.8
Mean solstitial zen. dist. of the sun's centre, June 11, 1690, o. s. 27 59	29.6	Error of the line of collimation ..	+ 4.8
And for the zenith distance of Arcturus,		Mean solstitial zenith distance of the sun's centre, 1771 .....	28 17 2.6
The mean, January 1, 1690, o. s. 30° 39'	34"	And the same for 1772, gives, for	
Precession to June 11, 1690 .....	+ 8.4	The mean .....	28° 17' 13.4"
Mean zenith distance of Arcturus,		Sun's parallax .....	— 4.1
June 11, 1690 .....	30 39 42.4		28 17 9.3
Mean solst. zen. dist. of the sun's centre, June 11, 1690 .....	27 59 29.6	Nutation .....	— 8.7
Mean distance of Arcturus in declin. from the sun's centre, 1690 ... 2 40	12.8		28 17 0.6
		Error of the line of collimation ..	+ 4.8
		Mean solstitial zenith distance of the sun's centre, 1772 .....	28 17 5.4
And for that of Arcturus the same year,		Mean dist. in June 1690 2° 40' 12.8"	
Mean zen. dist. of Arcturus, Jan. 1, 1772 31° 22'	29.8"	Precession, &c. to June	
Precession to June 21, 1772 .....	+ 9	1772 .....	+ 26 16.4
Mean zen. dist. of Arcturus, June 21, 1772 31 22	38.8	Computed dist. in June	
Error of the line of collimation .....	+ 4.3	1772 .....	3 6 29.2
True mean zenith dist. of Arcturus, June 21, 1772 ..	31 22 43	Observed dist. in June	
Mean zen. dist. of ☉'s centre, June 21, 1772 28 17	5.4	1772 .....	3 5 37.6
Mean dist. of Arcturus in declination from ☉'s centre, June 21, 1772 .....	3 5 37.6	Diminution of obliquity in 82 years .....	51.6

From the foregoing observations it appears that, at the summer solstice of the year 1690, Arcturus was 2° 40' 12".8 to the south of the sun's centre in declination: the motion of the star in declination, from that time to the summer solstice of the year 1772, including its proper motion, is 26' 16".4. Arcturus therefore, in 1772, should have been 3° 6' 29".2 to the south of the sun's centre,

if the angle of the ecliptic and equator had not varied: but that distance was found by actual observation to be  $51''.6$  less. By so much therefore must the obliquity of the ecliptic have become less in an interval of 82 years; and consequently the variation in 100 years will be  $62''.92$ . If the observations of Arcturus be reduced to the solstice of 1771, and the zenith distance of the sun's centre, as observed in that year, be made use of in the same manner, the variation of the obliquity in 81 years will be found  $= 48''.8$ , and in 100 years  $= 60''$ .

If the quantity of the arc of Mr. Flamsteed's instrument were accurately known, the observations which he made at the winter solstice in 1690 might be compared with later observations, in order to determine both the quantity of the obliquity in 1690, and also the variation since his time. Accordingly, Mr. H. endeavoured to determine the error of the arc of the instrument between  $28^\circ$  and  $75^\circ$  of zenith distance, and proceeded in the following manner. He computed several observations of the stars  $\zeta$  Tauri,  $\eta$  Pleiadum,  $\eta$  and  $\mu$  Geminorum, and  $\phi$ ,  $\sigma$ , and  $\circ$  Sagittarii, as observed by Mr. Flamsteed in the years 1690, 1691, and 1692, and reducing them to the years 1760 and 1766; he compared the differences of declination between those stars, resulting from Mr. Flamsteed's observations, with the differences given by the places of the same stars, as settled by Dr. Bradley in 1760, and also by actual observations of the same stars made at Shirburn castle in 1766; and by combining these differences together, he found that the whole arc of  $90^\circ$  was too short by  $43''$ . Supposing the error to be uniform, the proportional part of this quantity, thus found for the solstitial zenith distance of the sun in June  $= 13''.4$ , is nearly confirmed on the authority of Mr. Flamsteed himself, who, in the prolegomena to the 3d volume of the *Historia Cœlestis*, where he is deducing the latitude of the Royal Observatory at Greenwich, and the quantity of the obliquity in 1690, from his own observations, allows the zenith distances at  $28^\circ$ ,  $36^\circ$ , and  $40^\circ$ , on his instrument, to be too small by  $15''$  and by  $20''$ , at  $75^\circ$ . Mr. H. therefore computed the observations of the sun, made from November 30 to December 20 of 1690, which, reduced to the solstice, are as in the following table; to which are subjoined the observations made by himself at Oxford, at the winter solstice of 1771.

Of the former, the mean is . . . . .	$74^\circ 58' 25.9''$	Of the latter, the mean is . . . . .	$75^\circ 13' 17.3''$
Error of the line of collimation . .	$-1 \quad 10$	Sun's parallax . . . . .	$-8.5$
	$74 \quad 57 \quad 15.9$		$75 \quad 13 \quad 8.8$
Sun's parallax . . . . .	$-8.5$	Nutation . . . . .	$+7.9$
	$74 \quad 57 \quad 7.4$		$75 \quad 13 \quad 16.7$
Nutation . . . . .	$-9.6$	Error of the line of collimation . .	$+4.8$
Mean solstitial zenith distance of		Mean solstitial zenith dist. of the	
the Sun's centre, Dec. 1690 . .	$74 \quad 56 \quad 57.8$	sun's centre, December 1771	$75 \quad 13 \quad 21.5$

The mean obliquity of the ecliptic resulting from the zenith distances, as observed at the two solstices in 1690, by applying the known latitude of the

place, will be found to be widely different, if no correction be applied for the error of the instrument.

June, zenith distance . . . . .	—27° 59' 29.6"	Dec. zenith distance . . . . .	74° 56' 57.8"
Latitude of Greenwich . . . . .	51 28 38		—51 28 38
	23 29 8.4		23 28 19.8

But if the observations be corrected by the error of the instrument, the two results will be found to agree together as nearly as can be expected.

Thus, 27° 59' 29.6"	74° 56' 57.8"	Or, if the obliquity be required independent of a knowledge of the latitude of the place, it will be found to be =
+13.4	+35.8	
— 27 59 43	74 57 33.6	
51 28 38	— 51 28 38	
Obliquity 23 28 55	23 28 55.6	23° 28' 55".3. Thus,

December . . . . .	74° 57' 33.6"	By comparing the observations at the summer solstices of 1771 and 1772, with those at the winter solstice of 1771, it appears that the mean obliquity was, about the beginning of
June . . . . .	—27 59 43	
Difference . . . . .	46 57 50.6	
Mean obliquity 1690,		
½ difference . . . . .	23 28 55.3	

the year 1772, = 23° 28' 9".4 and 23° 28' 8". Mr. H. supposes therefore the mean obliquity to be 23° 28' 8" at the beginning of the present year; and consequently the obliquity has diminished, by his observations, 47" in 81 years, since Mr. Flamsteed's time, or at the rate of 58" in 100 years, a quantity which will be found nearly at a mean of the computations framed by Mr. Euler and Mr. de la Lande, on the principles of attraction.

*XV. New Observations on Vegetation. By Mr. Mustel of the Acad. of Sciences at Rouen. Translated from the French. p. 126.*

Many celebrated writers, induced by the analogy which they observed between the vegetable and animal kingdoms, have admitted the circulation of the sap in the one, in a similar manner to the circulation of the blood in the other. This important point of vegetable economy produced a diversity of opinions, and has not yet been sufficiently cleared up. Dr. Hales, in his *Vegetable Statics*, does not seem to embrace the system of the circulation of the sap; nor does he prove the contrary.\*

Mr. Du-Hamel, in his *Physiology of Trees*, contents himself with relating what has been said for or against this opinion; but though he sufficiently hints

\* Il ne prouve pas contre. This certainly is a mistake. Dr. Hales, in the 4th chapter of his *Physical Statics*, not only declares openly against the doctrine of the circulation of the sap, and overturns the arguments alleged in favour of this opinion; but he produces several new experiments, which prove directly the impossibility of such a circulation. (See p. 144, &c.) His reasons have been thought so convincing, that the system of the circulation in plants has been ever since exploded in England; and that they have had a similar effect abroad, appears from the following quotation from a book of the ingenious Mr. Bonnet, F. R. S. of Geneva, entitled *Recherches sur l'Usage des Feuilles*, printed in 1754, p. 269. 'Pour moi, persuadé de la fausseté de cette opinion (que la seve circuloit comme le sang) par les expériences de M. Hales (ch. 4) &c.' M. M.—Orig.



that he does not believe it true, he determines nothing about it. The friends of the circulation in plants have never been able to find in them any thing analogous to that powerful organ, which is the promoter of it in animals; for want of such an organ, they were forced to imagine valves and paps in the lymphatic vessels of plants, by means of which the liquors once introduced into the sap vessels were supposed to be hindered from going back; but unfortunately no body has ever been able to discover these valves and paps, so different from the simple contrivances, by which nature is used to arrive at her ends.

An experiment, which Mr. M. made, and of which he proposes giving an account in this paper, throws a great light on this question, as well as on several others; and the conclusions deducible from it appear to him decisive. On the 12th of January, he placed several shrubs in pots against the windows of his hot-house, some within the house, and others without it. Through holes made for this purpose in the panes of glass, he passed a branch of each of the shrubs, so that those on the inside had a branch without, and those on the outside one within; after this, he took care that the holes should be exactly closed and luted. This inverse experiment, he thought, if followed closely, could not fail affording sufficient points of comparison, to trace out the differences, by the observation of the effects.

The 20th of January, a week after this disposition, all the branches that were in the hot-house began to disclose their buds. In the beginning of February, there appeared leaves, and towards the end of it shoots of a considerable length, which presented the young flowers. A dwarf apple-tree and several rose-trees, being submitted to the same experiment, showed the same appearance then, as they commonly put on in May; in short, all the branches which were within the hot-house, and consequently kept in the warm air, were green at the end of February, and had their shoots in great forwardness. Very different were those parts of the same tree, which were without, and exposed to the cold. None of these gave the least sign of vegetation; and the frost, which was intense at that time, broke a rose-pot placed on the outside, and killed some of the branches of that very tree; which on the inside was every day putting forth more and more shoots, leaves, and buds, so that it was in full vegetation on one side, while frozen on the other.

The continuance of the frost occasioned no change in any of the internal branches. They all continued in a very brisk and verdant state, as if they did not belong to the tree, which, on the outside, appeared in a state of the greatest suffering. On the 15th of March, notwithstanding the severity of the season, all was in full bloom. The apple tree had its root, its stem, and part of its branches, in the hot-house. These branches were covered with leaves and flowers; but the branches of the same tree, which were carried to the outside,

and exposed to the cold air, did not in the least partake of the activity of the rest, but were absolutely in the same state which all trees are in during winter. A rose-tree, in the same position, showed long shoots with leaves and buds; it had even shot a vigorous branch on its stalk, while a branch which passed through to the outside had not begun to produce any thing, but was in the same state with other rose-trees left in the ground. This branch is 4 lines in diameter, and 18 inches high.

The rose-tree on the outside was in the same state; but one of its branches drawn through to the inside of the hot-house, was covered with leaves and rose-buds. It was not without astonishment that Mr. M. saw this branch shoot as briskly as the rose-tree which was in the hot-house, whose roots and stalk, exposed as they were to the warm air, ought, it should seem, to have made it get forwarder than a branch belonging to a tree, whose roots, trunk, and all its other branches were at the very time frost-nipt. Notwithstanding this, the branch did not seem affected by the state of its trunk; but the action of the heat on it produced the same effect, as if the whole tree had been in the hot-house.

It would be useless to give an account of the diary he kept throughout the course of this interesting experiment. It may be sufficient to observe, that the walk of nature was uniformly the same. The interior branches continued their productions in a regular manner, and the external ones began theirs at the same time, and in the same manner as they would have done, had they been left in the ground. The fruits of the interior branches of the apple-tree were, in the beginning of May, of the size of nutmegs; while the blossoms but just began to show themselves on the branches without. Mr. M. observed that 3 of the flower-buds of the apple-tree had been gnawed off by a snail in such a manner, that all the petals and stamens had disappeared, being eaten up close to the calyx. This not having been entered by the snail, the basis of the pistillum and the embryo were preserved. He took it for granted that these flowers would bear nothing; but he was soon convinced of his mistake. Almost all of them bore fruit; the apples were perfectly formed, and 6 or 7 pretty large ones too were seen on each bunch. On the other hand, the snail had spared some other bunches, doubtless because more difficult to be got at; but out of 10 or 12 flowers in each bunch, not above 1 or 2 showed any signs of fruit. This suggested the idea, that when the flowers of trees are full blown, the prevention of the natural fall of the petals and stamens gives a greater assurance of the fructification: and on several times repeating the following experiment, he convinced himself that it did so. In imitation of the snail, he cut with his scissars the petals of apple, pear, plum, and cherry blossoms, close to the calyx. Almost

every one of those, which were thus cut, succeeded, while several of the neighbouring flowers miscarried.

Thus did a snail teach him how to render a tree fruitful; nor is it the first time that animals have been the instructors of mankind. However, this process is not very practicable in a large orchard; but it might be adopted in an espalier; in which one would choose to procure a great deal of fruit from trees of the best sort. It may indeed be questioned, whether the suppression of the stamens would not render the fruit barren; and in fact he found, that though the flowers of the dwarf apple-tree, whose petals and stamens were eaten up by the snail, gave apples equally large and beautiful, and that when he came to open them, he found the capsules formed as usual at the centre of them; yet they were entirely empty, without the least appearance of a pip. Absolute fructification therefore did not take place; since botanists, with reason, call nothing fruit but the seed, which contains the gerimen, which is to perpetuate the species. All the other parts, being only intended to co-operate in the formation and preservation of the seeds, perish of course, when once the seeds are come to maturity and perfection, and the work of nature fulfilled. Another remarkable thing in these apples is, that in the upper part there was found a much deeper cavity than usual. It was 8 or 9 lines deep. The orifice of this cavity was bordered by 5 tubercles, indented and somewhat elevated; but there was no vestige of the calyx, which it is well known remains always to the upper part of apples and pears, and is commonly called the eye.

But to return to the first experiment; the consequences of which, as before described, seem to prove, 1. First that the circulation of the sap does not take place in plants, as the circulation of the blood in animals. This may be deduced from the following observations. The tree in the hot-house went through all its changes during the winter, and the branch exposed to the open air underwent none; consequently the sap, which was in action in the root, stock, and head, of the tree, did not circulate through the branch without; which had no share in the vegetation of the roots and trunk. It might indeed be argued, that the cold air, to which this branch was exposed, stopped the circulation, and therefore that the first experiment would not be decisive: but the inverse of it seems fully so. The tree placed on the outside of the hot-house continued, during the whole winter, in the state of numbness, natural to all trees, which are exposed at that season; but one of its branches, which was in the hot-house, put forth successively its buds, leaves, blossoms, and fruits. While therefore the root of the tree, to which this branch belonged, was in the ground so frozen, that the pot itself in which it stood was broken by it, while the stock and top of the tree were so covered over with ice, that many of the branches were killed; this branch

alone did not in the least partake of the common state of numbness and suffering, but was on the contrary in full vegetation. The sap in it must have been extremely rarefied, and in very quick motion, while that of the tree was greatly condensed, and in total inaction. How is it possible to conceive a circulation of the sap from such a frozen root and stock, to a branch full of vigour, and loaded with leaves and flowers? Surely this experiment must appear conclusive against the system of circulation; since in this case it could at best only be admitted to have taken place in the vegetating branch; and that would very improperly be termed circulation, which should be confined to one limb.

2. This experiment proves, that each part of a tree is furnished with a sufficient quantity of sap to effect the first production of buds, flowers, and fruits. There is little probability that the branch drawn into the hot-house should have derived its sap from the roots of the tree: as they, at that time, lay in a very small quantity of earth, rendered extremely hard and dry by the frost, they could have but little fluid to spare; and even this, considering the congealed state of the lymphatic vessels of the stock, could have found no passage to the branch. This branch must of course have been enabled to continue its vegetation by the quantity of sap with which it was provided, the consumption of which must have been supplied at the first breaking of the frost. This truth, now demonstrable by experience, had been pointed out before by a multiplicity of other facts. Every body may have observed that a tree, which has been blown down in autumn, though separated from its trunk, begins the same vegetation, that it would have done if it had remained standing. Its buds open, it bears leaves, and even shoots, which sometimes are very long, and must be the effects of the sap it contained. It is true indeed that this appearance does not continue long, because the provision of sap once exhausted, without being renewed, every thing must of necessity perish. An effect of the like kind often deceives us in trees that have been newly planted, and in scions, which produce flowers and even fruits, without ever having taken root. But in this case, the symptoms which would seem to promise life, are on the contrary the forerunners of death; because the leaves, being from their nature the most powerful organs of transpiration and dissipation, the graft is the more readily exhausted, when there is no root to furnish it with a fresh supply of nutritive juices.

3. This experiment proves that it is heat which unfolds the leaves, and produces the other parts of fructification, in the branch exposed to its action. Autumn is the time in which nature employs itself, as it were clandestinely, under the cover of the leaves, in forming the buds, which contain the rudiments of the leaves, blossoms, and fruits, that are to be produced in the course of the succeeding summer. These buds prepare and work themselves out, during the winter, under the rough coats that are destined to preserve them

from the injuries of the weather. As soon as the warm weather in the spring begins to be felt, the buds open, and their coats, which then become useless, drop off, and give place to the productions which they contained and preserved. Immediately after this, the blossoms, flowers, and fruits make their appearance. This is the usual operation: but in the case before us, nature was, as it were, surprized by art; what she should not have done till spring, she did in the winter, because the heat of the hot-house produced that expansion, which, according to the natural course, ought to have been effected by the rays of the sun darting less obliquely than before on the horizon. There is no doubt but it is to heat, either natural or artificial, that this expansion is owing; and the experiment proves that it is only in that part of the tree, which is exposed to the effect of heat, that the sap, which in every other part remains torpid and inactive, is put into motion, and produces vegetation. From this it appears, that the vegetable economy is different from the animal, and that those who endeavoured to establish the circulation in both, carried their analogy too far.

This fact, now established, furnishes a good reason why in the tapping of the maple and sugar birch trees, so much liquor runs out on one side, and none at all on the other. It is well known that, if during the time of a frost, or a summer's day, towards noon, you bore a hole on the side of the maple tree exposed to the south, you will get a great quantity of liquor from it; and that if you bore the north side at the same time, you will not get a drop. The cause of this evidently appears from what has been said. We likewise see why trees exposed to the south lose a great many of their branches, and sometimes die altogether, in the course of a severe winter; while trees of the same sort, but placed to the north, or in some other exposition, will stand the hardest frosts. This is particularly remarkable in the evergreens, whose resinous and oily sap being liquefied by the heat of the sun, the tree cannot escape suffering a great deal, whenever it is surprized in that state by the night frosts. Those observers who attend to this, and know how well pines, firs, and bays succeed, when planted on the back of mountains exposed to the north, will take care not to place such kind of trees in a southern aspect, in hopes of their succeeding better by it.

*XVI. Actual Fire and Detonation produced by the Contact of Tin-foil, with the Salt composed of Copper and the Nitrous Acid. By B. Higgins, M. D., p. 137.*

Several pieces of thin sheet copper, placed vertically, and at a small distance from each other, in the strong nitrous acid diluted with half its quantity, or more, of water, and suffered to remain in a close vessel, till the acid is saturated, afford a crystalline bluish green salt, which is to be separated from the undissolved copper and the superfluent green liquor, and kept in a well corked

bottle; because, on exposure to the air, it deliquesces. This salt, taken moist, but not very wet, and beaten to the fineness of basket sea salt, in a mortar, is to be strewed to the thickness of a shilling, on a piece of tin foil, 12 inches in length, and 3 in breadth. Then the foil is to be instantly rolled up, so as to include the salt, as it lay, between the coils. The ends are to be shut by pinching them together, and the whole is to be pressed flat and close.

All this being done as quickly as possible, the first phenomenon is—A part of the salt deliquesces. 2d. This part impregnated with tin, changed in colour, and of a thicker consistence, begins to froth from the ends of the coil. 3d. A strong frothing, accompanied with moderate warmth. 4th. The emission of copious nitrous fumes. 5th. Heat intolerable to the fingers. 6th. Explosion and fire, which burst and fuse the tin foil in several places, if it be very thin.

After many conjectures and experiments, Dr. H. discovered a property in the cupreous salt, from which, and the known affinities of the bodies concerned, these appearances, however new and singular, may be accounted for. The cupreous salt, properly dried, and placed where it may receive a heat, not much greater than what the hand can bear, takes fire. The circumstances which favour this ignition, and contribute to produce it in the smallest degree of heat, concur in the following convenient method of trying the experiment. A piece of soft bibulous paper is to be dipped in the nitrous solution of copper, and dried before the fire 2 or 3 times alternately. Then it is to be approached towards the heat, as near as can be borne, by the hand which holds it, without pain: there, if it has been sufficiently dried, it will presently catch fire, and burn to a brown calx.

The easy ignition of the salt in a slight heat being thus ascertained, there is no room to doubt that the foregoing phenomena are produced in the following manner. The acid of the liquor, which moistened the salt, quits the copper, to unite with the tin, leaving the water to be imbibed by the contiguous salt of copper, which then dissolves, and acts briskly on the tin foil.

It is well known that the action of the nitrous acid on tin is always accompanied with considerable heat and effervescence, and that the solution of metallic salts in watry liquors is hastened by heat. In this experiment, the warmth generated by the first action of the cupreous solution, promotes the deliquescence of the crystallized salt. The union of the acid with the tin is rapid, not only as being assisted by heat, but on account of the great surface exposed; whence the strong frothing, and the extraordinary heat, by which the redundant moisture is carried away, and the undecomposed part of the cupreous salt, together with that lately formed with the tin, perfectly dried.

The heat generated on both surfaces of a large expanse of tin, is concentrated by closely coiling it into a small compass, and being retained by the various sur-

rounding laminæ of metal, it is necessarily accumulated to a quantity which, if we may judge from the touch, is more than sufficient to fire the dry cupreous salt. The salt formed with tin, and the nitrous acid, burns and sparkles in a red heat. Catching fire therefore, from the ignited cupreous salt, it burns with it, and assists in the detonation, which is common to all nitrous compositions in similar circumstances.

If the salt be very wet, there will not be much fire or explosion, because the heat will be dissipated before the salt can be sufficiently dried in every part. If the salt be not moist, it cannot commence the action which is necessary; and there will be no fire, because there can be no hasty solution of the tin to give the requisite heat. If the tin and salt be not coiled up in due time, there will be very little heat, and no fire; because the dissipation of the heat from a broad expanse, keeps pace with the generation of it; and as the moisture exhales quickly in this manner, there is none left to renew the action on the tin and consequent heat, when the proper time of coiling has elapsed.

A piece of tin foil, larger than that above described, cannot easily be managed; smaller pieces give less fire in the direct proportion of their surfaces, and the quantity of salt which they can, at the same instant, reduce to the required state of dryness. The sudden dissipation of the moisture appears the most curious of these phenomena. To render it the more observable, he made the following experiments: he placed a piece of tin foil, 12 inches long by 2 broad, loosely coiled, and standing vertically on the flattest end, in half a table-spoonful of the saturated solution of copper in the diluted nitrous acid; and found that scarcely 5 seconds elapsed, from the time when a brisk effervescence, accompanied with weak nitrous fumes, arose, till the liquor became a consistent mass, and sparks of fire issued from the coils of tin; which having attracted part of the solution above the common level, brought it into the condition in which it is readily dried, heated, and fired.

A like quantity of the same solution, kept in a strong boiling heat, does not acquire such consistence in a ten fold space of time. The hasty exhalation therefore, is not caused by the heat alone; neither does it seem to require any great surface. What else it is owing to, he commits a while to the examination of the curious.

XVII. *Extracts of some Letters, from Sir Wm. Johnson, Bart., to Arthur Lee, M. D., F. R. S., on the Customs, Manners, and Languages of the Northern Indians of America.* p. 142.

In all inquiries of this sort, we should distinguish between the more remote tribes, and those Indians, who, from their having been next to our settlements for several years, and relying solely on oral tradition for the support of their

ancient usages, have lost great part of them, and have blended some with our customs, so as to render it extremely difficult, if not impossible, to trace their customs to their origin.

The Indians did certainly live under more order and government formerly, than at present. This may seem odd, but it is true; for, their intercourse being with the lower class of our traders, they learn little from us but our vices; and their long wars, together with the immoderate use of spirituous liquors, have so reduced them, as to render that order, which was first instituted among them, unnecessary and impracticable. They do not at present use hieroglyphics; their figures being drawn, to the utmost of their skill, to represent the thing intended. For instance, when they go to war, they paint some trees with the figures of warriors, often the exact number of the party; and if they go by water, they delineate a canoe. When they gain a victory, they mark the handle of their tomahawk with human figures, to signify prisoners; and draw the bodies without heads, to express the scalps they have taken. The figures which they affix to deeds have led some to imagine, that they had alphabetical characters or cyphers. The fact is this: every nation is divided into tribes, of which some have 3, as the turtle, bear, and wolf; to which some add the snake, deer, &c. Each tribe forms a little community within the nation; and as the nation has its peculiar symbol, so has each tribe the particular badge from which it is denominated: and a sachem of each tribe being a necessary party to a fair conveyance, such sachem affixes the mark of his tribe to it, like the public seal of a corporation. With respect to the deed of 1726, of which you sent me the signatures, the transaction was in some measure of a partial nature. All the nations of the confederacy did not subscribe it; and those chiefs who did, neglected to pay due regard to their proper symbols; but signed agreeably to fancy, of which I have seen other instances. The manner I have mentioned is the most authentic, and conformable to their original practice.

As to the information which, you observe, I formerly transmitted to the governor of New-York, concerning the belt and 15 bloody sticks sent by the Missisagees, the like is very common; and they use these sticks, as well to express the alliance of castles, as the number of individuals in a party. The sticks are generally about 6 inches in length, very slender, and painted red if the subject be war. Their belts are mostly black wampum, painted red when they denote war. They describe castles sometimes on them, by square figures of white wampum; and in alliances, human figures holding a chain, which is their emblem of friendship, and each figure represents a nation. An axe is also sometimes described, and always imports war: the taking it up, being a declaration of war; and the burying it, a token of peace.

With respect to your questions concerning the chief magistrate, or sachem,



and how he acquires his authority, &c. I am to acquaint you, that there is, in every nation, a sachem, or chief; who appears to have some authority over the rest, and it is greatest among the most distant nations. But in most of those bordering on our settlements, his authority is scarcely discernible, he seldom assuming any power before his people. And indeed this humility is judged the best policy; for, wanting coercive power, their commands would perhaps occasion assassination, which sometimes happens. The sachems of each tribe are usually chosen in a public assembly of the chiefs and warriors, whenever a vacancy happens by death or otherwise; they are generally chosen for their sense and bravery, from among the oldest warriors, and approved of by all the tribe; on which they are saluted sachems. There are however several exceptions; for some families have a kind of inheritance in the office, and are called to this station in their infancy.

The chief sachem, by some called the king, is so, either by inheritance, or by a kind of tacit consent, the consequence of his superior abilities and influence. The duration of his authority depends much on his own wisdom, the number and consequence of his relations, and the strength of his particular tribe. But even in those cases where it descends, should the successor appear unequal to the task, some other sachem is sure to possess himself of the power and the duties of the office. I should have observed, that military services are the chief recommendations to this rank. And it appears pretty clearly, that heretofore the chief of a nation had, in some small degree, the authority of a sovereign. This is now the fact among the most remote Indians. But as, since the introduction of fire arms, they no longer fight in close bodies, but every man is his own general, I am inclined to think this has contributed to lessen the power of a chief. This chief of a whole nation has the custody of the belts of wampum, &c. which are as records of public transactions: he prompts the speakers at all treaties, and proposes affairs of consequence. The chief sachems form the grand council; and those of each tribe often deliberate on the affairs of their particular tribes. All their deliberations are conducted with extraordinary regularity and decorum. They never interrupt him who is speaking; nor use harsh language, whatever may be their thoughts. The chiefs assume most authority in the field; but this must be done, even there, with great caution; as a head warrior thinks himself of most consequence in that place.

The Indians believe in, and are much afraid of witchcraft: those suspected of it are therefore often punished with death. Several nations are equally severe on those guilty of theft, a crime indeed uncommon among them: but in cases of murder, the relations are left to take what revenge they please. In general, they are unwilling to inflict capital punishments, as these defeat their grand political object, which is, to increase their numbers by all possible means.

On their haunts, as on all other occasions, they are strict observers of meum and tuum; and this from principle, holding theft in contempt; so that they are rarely guilty of it, though tempted by articles of much value. Neither do they strong attempt to seize the prey of the weak; and I must do them the justice to say that, unless heated by liquor, or inflamed by revenge, their ideas of right and wrong, and their practices in consequence of them, would, if more known, do them much honour. It is true that, having been often deceived by us in the purchase of lands, in trade, and other transactions, many of them begin now to act the same part. But this reflects most on those who set them the example.

As to your remark on their apparent repugnance to civilization, I must observe, that this is not owing to any viciousness of their nature, or want of capacity; as they have a strong genius for arts, and uncommon patience. I believe they are put to the English schools too late, and sent back too soon to their people, whose political maxim, Spartan like, is to discountenance all pursuits but war, holding all other knowledge as unworthy the dignity of man, and tending to enervate and divert them from that warfare on which they conceive their liberty and happiness depend. These sentiments constantly instilled into the minds of youth, and illustrated by examples drawn from the contemptible state of the domesticated tribes, leave lasting impressions; and can hardly be defeated by an ordinary school education.

I wish my present leisure would allow me to give you as many specimens of their language as would show that, though not very wordy, it is extremely emphatical; and their style adorned with noble images, strong metaphors, and equal in allegory to many of the eastern nations. The article is contained in the noun, by varying the termination; and the adjective is combined into one word. Thus of echin, a man, and gowana, great, is formed echingowana, a great man. Caghyunghaw is a creek, caghyungha a river, caghyunghaowana a great river; caghyunghewo a fine river. Haga the inhabitants of any place, and tierham the morning; so, if they speak of eastern people, they say tierhans-aga, or people of the morning. Eso is expressive of a great quantity, and esogee is the superlative. The words goronta and golota, which you mention, are not of the six nations, but a southern language. It is curious to observe, that they have various modes of speech and phrases peculiar to each age and sex, which they strictly observe. For instance, a man says, when he is hungry, cadagcariax, which is expressive both of his want and of the animal food he requires to supply it; whilst a child says, in the same circumstances, cautsore, that is, I require spoon-meat.

There is so remarkable a difference in the language of the six nations from all others, as affords ground for inquiring into their distinct origin. The nations

north of the St. Lawrence, those west of the great lakes, with the few who inhabit the sea coasts of New England, and those again who live about the Ohio; notwithstanding the respective distances between them, speak a language radically the same, and can in general communicate their wants to one another; while the six nations, who live in the midst of them, are incapable of conveying a single idea to their neighbours, nor can they pronounce a word of their language with correctness. The letters *m* and *p*, which occur frequently in the other languages, are not in theirs; nor can they pronounce them but with the utmost difficulty. There is indeed some difference of dialect among the six nations themselves; but this is little more than what is found in all the European states.

*XVIII. Of some curious Fishes, sent from Hudson's Bay. By Mr. J. Reinhold Forster, F. R. S. p. 149.*

The governor and committee of the Hudson's Bay Committee presented the R. S. with a choice collection of skins of quadrupeds, many fine birds, and some fish, collected by their servants at the several ports in Hudson's Bay; the committee of the R. S., for examining and describing these curiosities, referred them to Mr. F. for examination. And he here adds the following observations on the fish from that place.

The 4 kinds of Hudson's Bay fish are, the sturgeon, the burbot, the gwiniad, and a new fish called the sucker at Hudson's Bay. The sturgeon was about 14 inches long, and therefore seems to be a young fish: as it is likewise observed in the list written by the gentleman who sent this fish from York fort. Its nose is very long and slender, terminating in a point; the eyes are small; under the projecting snout, before the mouth, are 4 beards or cirrhi, placed nearly in the same line, and not by pairs as in some other species of sturgeon. The mouth is beneath, nearly opposite the eyes, toothless, cartilaginous, semilunar when in its natural position, but round when open; on each side are 2 nostrils. The whole head is depressed, and very nearly quadrangular; the whole body pentagonal, and tapering towards the tail; the whole skin tough, covered with 5 rows of uncinated scales; the dorsal series consists of 14 large roundish scales, and a single one behind the dorsal fin; each of the lateral rows has 35 oblique scales; in the 2 ventral rows are 9 roundish strong scales between the pectoral and ventral fins: one scale is behind the vent, and another behind the anal fin.

The fish, according to this description, seems to come the nearest to that species of sturgeon which he described in the Phil. Trans., vol. 57, [abridged vol. xii.] in his Specimen Historiæ Naturalis Volgensis, N° 10, under the name of *acipenser ruthenus major, rostro elongato acuminato, paululum supino*, and which the Russians call *Sevruga*. Kramer, in his *Elenchus Vegetabilium et*

*Animalium Austriæ*, p. 383, is the only writer who takes notice of this species; he calls it *acipenser rostro acuto, corpore tuberculis spinosis aspero*: the inhabitants of Austria call it *shirk*, a name they have no doubt borrowed from the Slavonian name *sevruga*. The famous painter and traveller Cornelys de Bruyn mentions this kind of fish, but in so superficial a manner, that one plainly sees he was little, if at all, used to discussions in points of natural history. Had de Bruyn examined the *sevruga*, he would certainly have found it materially different from the *stoer* or *assetrina*, i. e. the common blunt nosed sturgeon of Germany and the Baltic. Mr. F. supposes the English sturgeon, from Pennant's description, and the drawing in the British Zoology, illustrated by plates, tab. 89, to be the same with this kind from Hudson's Bay, and with the *sevruga* of the Russians, and the *shirk* of the Austrians. The true sturgeon, which gave the name to the whole genus,\* he thinks an unknown fish in England. The species of sturgeons are more numerous than one is at first aware of; and it would therefore be of some utility, that persons, who have an opportunity of examining all the various kinds at Vienna, and in Russia, might do it with more care than has hitherto been done. Mr. Klein, a very ingenious naturalist, has enumerated 10 sturgeons, in his 4th *Missus Piscium*, p. 11—16; and Count Marsigli, in his splendid work about the Danube, tom. 4, gives the names of at least 6 sturgeons, but the characters are not sufficiently settled in both these works. Klein saw only 2 kinds of sturgeons, and a 3d in spirits; and Count Marsigli was not enough of a naturalist to give adequate descriptions of these fish.

The 2d of the Hudson's Bay fish, is called, by the wild natives of that country, *inorthy*, and is no other than our common burbot, *gadus lota*, Linn. only vastly superior in size. The descriptions given of this fish, in the British Zoology, is entirely corresponding with this specimen, so that it would be superfluous to presume to make any additions to it. However, after a most minute examination, Mr. F. could find no more than 6 branchiostegous rays in the two specimens from Hudson's Bay, of which Mr. Pennant mentions 7 in the English burbot, and Artedi as many in his specimens. This great naturalist seems likewise to be right, when he observes that the *cirrh*i, or beards on the end of the nose, are the valves to one of the nostrils; for Mr. F. found that these beards, on their under side, opened into a hole corresponding with the lower nostril. Mr. A. Graham, the collector of the natural history specimens at Severn river in Hudson's Bay, observes, that these fish constantly swim close to the ground, and are extremely voracious; for he represents them as not content with

\* The Germans call this fish *stoer*, from the old Teutonic word *stor* or *stühr*, which signifies great, as this fish grows to a very large size. Thus likewise the Scotch call the tunny, mackrel *sture*. Vide Mr. Pennant's Tour in Scotland, p. 192.—Orig.

devouring every fish\* they can overcome, but likewise feeding on putrefying deer, or other carrion that comes in their way; even stones are sometimes swallowed to satisfy their insatiable appetite, of which Mr. Graham was himself a witness, having taken a stone of a pound weight out of the stomach of this fish. The pike is often obliged to fall a victim, together with the trout, tickomeg, and others, to this rapacious fish. After sunset, it is caught by a night hook. It does not masticate its food before deglutition. Its roe and liver are reckoned a delicacy, when fresh caught; but they turn rancid and oily in a few days, though kept frozen solid all the time. At Hudson's Bay this fish is thought to be dry and insipid; its weight is from 1 to 8 pounds.

The 3d species of fish, from this cold climate, is by the natives called tickomeg, and is our gwiniad or *salmo lavaretus*, Linn.; only the size is somewhat larger; for the greatest specimen sent over measures 18 inches from the head to the tip of the tail, is  $4\frac{1}{2}$  inches deep, and not above an inch and  $\frac{1}{4}$  thick. This fish differs in no circumstance from our gwiniad, but the length. Mr. Pennant mentions in the British Zoology, vol. 3, p. 269, a ferra or gwiniad from Switzerland 15 inches long, as an uncommon size;† the Hudson's Bay fish, as I have before observed, is 18 inches long, and  $4\frac{1}{2}$  inches its greatest depth. The great abundance of food, and the small number of inhabitants, who let the fish grow up undisturbed, are perhaps the causes of their uncommon size. They weigh from  $1\frac{1}{4}$  pound to 3 pounds, says Mr. Graham; but the fish Mr. F. examined must, when fresh, have weighed more. These fish abound in the river Severn in Hudson's Bay, from its origin in the great lakes to its mouth, where it empties itself into the bay. The natives catch 5 or 6 hundred a day, by means of wears which they contrive in the river: they will not take bait, and are poor at the breaking of the ice in the river. In the middle of the summer, after a gale of wind, they are often found thrown up into the marshes, and on the shoals, where they remain at the recess of the water and abating of the wind, and serve as food to numbers of crows. The inhabitants of Hudson's Bay think this fish very sweet, and good to eat, contrary to the opinion of many Europeans.

The 4th and last fish brought from Hudson's Bay, is there called a sucker; because it lives by suction, according to Mr. Graham's account, who also says there are 2 varieties of this fish, both of a whitish colour, but one distinguished by a mixture of beautiful red. In the smallest of 2 specimens brought over, a broad stripe of red could be observed all along the *linea lateralis*. They are very numerous in the creeks and rivers, and troublesome in overburdening the nets.

\* This too is the fish that makes such havock in the lake of Geneva. P.—Orig.

† However, the gwiniads of Lapland, a similar climate to that of the Hudson's Bay, are vastly large. Brit. Zool. 3, 297, note.—Orig.

They are not deemed a palatable food, being very soft, and full of small bones. They weigh from one-half to  $2\frac{1}{4}$  pounds.

The above is literally what Mr. Graham says of this fish, and all that is known of its natural history. Examining it carefully, Mr. F. found it was a new species of the genus of cyprinus, or carp. The head is broader than the body, gradually decreasing towards the nose, full of elevations and tubercles, nearly quadrangular, and not scaly. The mouth is quite under the head, as in the loricariæ, when shut, semilunar; when open, round; not far from the extremity of the snout, and included in small round lips. To the under lip is fixed a bilobated, beard-like, papillose caruncula; it has no teeth. The eyes are large, but the colour of the iris could not be determined. The number of the branchiostegous rays is 3. The body is flat, tapering towards the tail, and scaly. The greater specimen measures very near 15 inches from the nose to the extremity of the tail; next to the head it is nearly 2 inches thick, about the dorsal fin  $1\frac{1}{4}$  inch; its greatest depth before the ventral fins is  $2\frac{1}{4}$  inches. On the snout are about 5 round prominent tubercles; 2 nostrils are found on each side, the largest next before the eye is kidney-shaped. The covers of the gills are double, and divided; the head has several sutures; over each eye, in a cavity, are 2 longitudinal ones, joined opposite the nostrils by a still shorter transverse one; on the covers of the gills are 2, on each side 1, beginning near the lobes of the caruncula of the under lip, and going up arched towards the eye. Near the extremity of the snout begins on each side a longitudinal one; it passes under the eye, and mounts in a curvature behind it; then it goes on straight to the end of the head, where it again gets downwards, and joins the lateral line. Where the head joins to the body, these two sutures are connected by a transversal one, which, as it were, separates the head from the body. The lateral line at first descends from the head, but then runs on straight, rather nearer the back than the body, to the beginning of the tail. The scales are small near the head and back, increasing in size towards the middle and tail, close to which they are again smaller. The dorsal fin is placed somewhat behind the equilibrium of the fish, rhomboidal, and consisting of 12 strong branched rays. The pectoral fins are lanceolated, fixed under the covers of the gills, and have 17 rays. The ventral fins have 10 or 11 rays, and are placed in the middle of the belly, and under the dorsal fin. The anal fin consists of 8 branched strong rays. The tail is somewhat forked or concave, and consists of 17 rays.\*

\* Dr. Forster's English description of this fish (*Cyprinus catostomus*,) being very complete, of course renders the Latin one unnecessary, which is therefore omitted.

XIX. *Experiments on the Different Kinds of Marl found in Staffordshire. By Wm. Withering, M.D. p. 161.*

Number.	Description.	Quantity of cal- careous earth, in half a dram, as separated by the nitrous acid, and precipitated by mild fixed alk.	What was left after the foregoing separation, was no longer acted on by the nitrous acid; but being	One dram of each of the marls being calcined, weighed	The calcined marls put into water, produced		
		Grains.		Grs. Lost grs.	Burnt to		
1 Red and blue intermixed, in small friable lumps		1	Mixed with water, became	52	8	Red brick.	No effect.
2 Red, in small friable lumps		0½	Uniform and plastic.	53	7	Red brick.	No effect.
3 Grey, in large hard lumps		5	Uniform and plastic.	49	11	Soft yellow brick.	Weak lime wat.
4 Red, hard, compact		3	Plastic, but a little gritty.	50	10	Red brick.	No effect.
5 Red, with grey spots, in large hard lumps		8½	Uniform and plastic.	48	12	Hard grey stone.	Lime water.
6 Light grey, like a grit stone		8	Plastic.	51	9	Soft and stony.	Lime water.
7 Brown, friable, in large lumps		18	Gritty, no union.	46	14	Soft stone.	Lime water.
8 Red, in large friable lumps		14	No union.	48	12	Soft stone.	Strong lime wat.
9 Brownish white, very hard, like calca. incrustations		16	Plastic, but a little gritty.	43	17	Soft stone.	Strong lime wat.
10 Lead colour, friable, flaky		14½	No union, gritty.	48	12	Soft stone.	Strong lime wat.
11 Brown grey, very hard, in irregular lumps		16	No union, gritty.	40	20	Soft stone.	Strong lime wat.
12 Lead colour, in powder and in small hard lumps		20½	No union, gritty.	29	31	Powdery.	Strong lime wat.
			Uniform and plastic.				

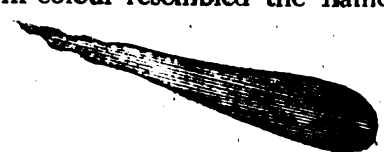
Half a dram of the marls being put into similar glass cups, 2 drams of nitrous acid being added to each glass, they all effervesced; N° 1 and 2 the least, N° 12 the most. The effervescence having ceased, and 6 drams of rain water being added to each glass, the liquors were all filtered, and after filtration, changed violet paper to a red colour. To the filtered colours was gradually added mild fixed alkali, sufficient to saturate the acid, and precipitate all the earth it had dissolved. The precipitated earth being washed in rain water, till free from all saline matter, weighed, when dry, as in the 3d column. Column the 4th shows that, after the separation of the calcareous earth, there remained in N° 1, 2, 4, a red clay; in N° 12 a white clay; in N° 8 a red clay, and a small portion of sand; in N° 3 a whitish clay, with a portion of sand; in N° 6, 9, 10, 11, pure sand; and in N° 7 sand, with a small portion of clay. These residuums were all washed with rain water before they were burnt. The precipitated powders being mixed together, 82 grains of it put into a crucible, and calcined with a strong heat, lost 35 grains in weight. Rain water was poured on the calx; the next morning there was a pellicle on the surface of the water; it tasted strongly of lime, and let fall a calcareous earth, on the addition of mild fixed alkali. The marls were kept for some weeks in a dry place before they were used. They were all got out of marl pits in the neighbourhood of Stafford, except N° 12, which is found near the Duke of Bridge-water's

water's canal, in a powdery form, and when mixed with  $\frac{1}{4}$  part of clay is burnt to quick lime. All the above marls crack and fall to pieces when exposed to the weather.

The foregoing experiments were undertaken with a view to ascertain how far it would be advisable to attempt burning the marls of this country into quicklime, for the purposes of agriculture; they may likewise furnish us with some useful hints relative to the kind of marls proper to be used on different kinds of lands. Perhaps the calcarious earth united with clay, as in N<sup>o</sup> 1, 2, 4, &c. may be the best for light sandy soil; and N<sup>o</sup> 6, 9, 10, 11, where the calcarious earth is united with sand, the most eligible where the land is already stiff, and abounding with clay. How far the different quantities of fixable air, or other volatile parts, contained in each of the marls, as shown by the 5th column, will influence their preference in agriculture, must be left to the experience of the farmer to determine.

*XX. Of a Fiery Meteor, seen Feb. 10th, 1772; and also on some New Electrical Experiments. Dated Eccles, Berwickshire. By Patrick Brydone, Esq., p. 163.*

On Monday the 10th of Feb. 1772, exactly at 7 in the evening, as Mr. B. was riding through Tweedmouth, a village at the south end of Berwick-bridge, he observed that the atmosphere was suddenly illuminated in a very extraordinary manner. The light of the moon, which was about half full, seemed to be extinguished by the blaze; and he saw his shadow projected on the ground, and almost as distinct, and well defined, as in sun-shine. He turned round to see whence the light proceeded, when he beheld a long, bright flame, moving almost horizontally along the heavens. It was of a conical form, and from the base to the apex could not be less than 6 or 7 degrees; its height, when he first observed it, seemed to be about 50 degrees; but it descended gently, and appeared to burst about 5 or 6 degrees lower. Its course was from N. W. to S. E., and seemed to have an inclination to the horizon; but this might be only a deception. The base of the cone was rounded like a sphere; and apparently of about  $\frac{1}{4}$  of the diameter of the moon at her greatest height; but its light was brighter than that of the planet Venus, and in colour resembled the flame of burning camphor. Near the end of the tail there was a kind of waving motion, which, with the whole appearance, is endeavoured to be represented by the annexed figure. In about 10 or 12 seconds it seemed to burst, dividing into a number of small luminous bodies, like the stars in a sky-rocket, which immediately disappeared.



As Mr. B. had formerly observed explosions from meteors of this kind, he had presence of mind to pull out his watch, which has a 2d hand, to measure the



exact time the report should take in reaching him. He waited upwards of 4 minutes, which in his state of expectation appeared a much longer time; when, despairing of any report, he rode on, but had not got to the middle of the bridge, when he was stunned by a loud and heavy explosion, resembling the discharge of a large mortar, at no great distance, and followed by a kind of rumbling noise, like that of thunder. He examined his watch, and found that the sound had taken 5 minutes, and about 7 seconds, to reach him; which, according to the common computation of 1142 feet in a second, amounts to the distance of at least 66 miles. It did not occur to him to measure the duration of the light, which probably did not exceed 10 or 12 seconds; and during this short period, the length of the path, the meteor seemed to describe, could not be less than 30 degrees. He expected to have seen some account of this phenomenon from Newcastle, as, by its direction and distance, he imagined it had burst pretty near the zenith of that town; but no notice was taken of it in the newspapers there. About a week after, he mentioned what he had seen to Sir John Paterson of Eccles, who told him he was at that time on the road, between Greenlaw and his own house; and as he was riding to the south, he observed the meteor from its first appearance, which was about 3 or 4 seconds sooner than he had time to turn about and view it; and this perhaps is the reason that it appeared so much higher to him than it did to Mr. B. That gentleman observed, that when it first became luminous, it was almost vertical, but went off descending to the s. e., and had in other respects the appearance above described. He added, that some considerable time after the light disappeared, he heard a great report, which he took for a clap of thunder; for the interval was so long, that he did not imagine this sound had any connection with what he had seen.

Now, as this gentleman was at least 20 miles to the west of the spot where Mr. B. made his observation, and as the appearance and height of this meteor seems to have been nearly the same to them both, it is probable that it was at a very great distance from the earth, and much beyond the limits that have been assigned to our atmosphere. The smaller meteors, called falling stars, Mr. B. frequently observed from the mountain of St. Bernard, one of the high Alps; and last year he had the good fortune to see several of them from the highest region of mount Etna; an elevation still more considerable, and probably the greatest accessible one in Europe, and they always appeared as high, as when seen from the lowest grounds; so that probably the height of 2 or 3 miles, bears but a small proportion to the common altitude of these bodies.

From their frequent appearance during the last frost, Mr. B. was inclined to believe, that the air was then in a very favourable state for electrical purposes; but not being provided with a common machine, he bethought him of a whim-

sical one to supply the want of it. The back of a cat, it is well known, often exhibits strong marks of electricity; being therefore desirous to try what effect this might produce, when made use of instead of the glass globe, he cut a quantity of harpsichord wire into short pieces, of 5 or 6 inches, and tying them together at one end, made the other diverge like the hair of a brush. He took a large metal pestle of a mortar for a conductor, to the end of which he fixed the brush of wire; and insulated the whole, by placing it on a couple of wine-glasses. He then took a cat on his knee, and bringing her back under the wires, he began to stroke it gently. The animal continued in good humour for a few minutes, and he had the satisfaction to see that the conductor was so much charged, that it emitted sparks of a considerable force, and attracted strongly such light bodies as were brought near it; but the cat at last becoming uneasy, threatened to put an end to the experiment. The passage of the electrical fire, from the hair of her back to the small wires, occasioned, it seems, a disagreeable sensation, which she could not bear; so that turning about her head to defend her back, the tip of her ear happening to touch the conductor, and a large spark coming from it, she sprung away in a fright, and would not allow him to come near her more. However, after a long interval, the animal seeming to have forgotten her adventure, a young lady in company, less obnoxious to her than he was, undertook to manage her. Having first covered the back of this lady's hand with a piece of dry silk, that none of the electric fire communicated to the wires might be lost, she then began to stroke the cat as he had done, and the conductor soon after appeared fully charged: they drew large sparks from it; and if the animal would have continued quiet, he had no doubt that they should have showed many of the common experiments in electricity; but she soon became so outrageous, that they were glad to put an end to the operations, without any hopes of being able to repeat them, at least with the same instrument. In this dilemma he recollected, that a lady had told him, that on combing her hair, in frosty weather, she had often been sensible of a little crackling noise; and in the dark had sometimes observed small sparks of fire to issue from it. He proposed, therefore, that one of the young ladies would suffer the experiment to be made on her head, which she agreed to. The conductor was then insulated as before, and the lady having placed herself so, that the back part of her head almost touched the brush of wire, he desired her sister to stand behind her, on a cake of bee's wax; who, as soon as she began to comb the hair of the former, the conductor emitted sparks still of a larger size than those they had hitherto seen. The hair was extremely electric, and when the room was darkened, they could perceive the fire pass from it along the small wires to the conductor. The young lady who was on the wax, was not a little surprised to find, that the moment she began to comb her sister's hair, her own body became electric, dart-

ing out sparks of fire against every substance that approached her. They found however that these sparks were not strong enough to fire spirits. Mr. B. then coated a small phial, and soon charged it from the conductor; but afterwards he did it more completely from the hair itself in the following manner. He fixed a brush of small wires to the large one that went through the cork of the phial; and taking the phial in his hand, he followed every motion of the comb with the brush of wires; and, in the dark, could observe the fire pass by these wires into the bottle. In a few minutes he found it was highly charged; when taking a spoonful of warm spirits in his left hand, and with his right, which grasped the phial, bringing the hook of the great wire near the surface of the spirits, a large spark darted from it, gave him a smart shock, and at the same time set the spirits on fire.

The day following, he wanted to repeat the experiments; but as the weather was hazy, and the frost had greatly abated, they did not so well answer. However, from making them on several heads, he found that the stronger the hair, the greater was the effect; whereas soft flaxen hair produced little or no fire at all. These experiments were made in a warm, dry room, before a good fire, and at a time when the thermometer, in the open air, was at 6 or 7 degrees below the point of congelation. The hair, which succeeded best, was perfectly dry, and no powder or pomatum had been used on it for some months before.

*XXI. Of a Fossil lately found near Christ-Church, Hants. By the Hon. Daines Barrington, V. P. R. S. p. 171.*

The shining divisions on the surface of this stone, seem to be the scales of a fish, which Mr. B. conceives to be the *acus maxima squamosa*, engraved in Willoughby's History of Fish, tab. p. 8, and described by Ray, in his Synopsis Piscium, p. 109. It appears by the catalogue of English fossils, in the collection of Dr. Woodward, that a still larger specimen of the same sort was found in Stansfield quarry, near Woodstock, though Dr. Woodward could only procure a single scale, v. 2, p. 53, c. 24. Single scales from the same quarry are also to be seen in the noble collection of fossils, given by Mr. Brander, F. R. S., to the British Museum. Though this fish therefore is a stranger to our seas, yet its exuviae are by no means so to our cliffs and quarries.

P. S. Mr. Hunter, F. R. S., having seen the fossil at Crane-court, happened to dissect a beaver's tail very soon afterwards, which he showed, as bearing a strong resemblance to the scaly divisions in this specimen; Mr. B. however still thinks that the form of the scales in the *acus maxima squamosa* of Willoughby is still nearer to it, than those in a beaver's tail.

*XXII. Description of a Rare American Plant of the Brownæa kind; with some Remarks on this Genus. By Mr. Peter Jonas Bergius, F. R. S. p. 173.*

As the *Leucandendra*, *Bruniæ*, *Diosmæ*, *Phylicæ*, *Hermannia*, &c. are peculiar to Africa, so are likewise the *Varroniæ*, *Ehretia*, *Samyda*, *Malpighia*, *Cacti*, *Brownæa*, &c. peculiar to America, not having been found in any other country: at present Mr. B. confines himself to the last mentioned kind. Mr. Jacquin, during his botanical travels in America, founded this genus, in memory of Dr. Patrick Browne, the celebrated English botanist; but Jacquin found only one species of this genus; neither was Sir Ch. Linné hitherto acquainted with any more. Mr. B. has now specimens of a new species of this kind, which he received from Mr. Pihl, who gathered it in Portobello in America, which will afford an opportunity of exhibiting the whole genus of the *Brownæa*, and the specifical differences of it. If we compare Mr. Jacquin's description of his species with this, we see how carefully nature has observed the same order and position of the essential parts in both; a circumstance common to all natural genera. Mr. B. does not know whether this plant will vegetate and thrive in our stoves or green-houses; if it does, he is convinced it will make a beautiful appearance with its assemblage of purple or blood-red flowers.

*Genus Brownæa.*—1. *Brownæa (coccinea)* B. with separate umbellated flowers. *Brownæa coccinea*. Linn. Spec. Plant. 958. Jacquin, Hist. Stirp. Amer. 194, t. 121. Native of rocky and woody places.

2. *Brownæa (Rosa de monte)* B. with aggregate headed sessile flowers, with very long stamens. *Hermesias*. Loefling. Itin. p. 278. Native of mountainous places.

*Descr.* *Trunk* arboreous; *branches* torulose with a cinereous bark; *branchlets* (or common petioles) subalternate, cylindric, smooth, with a cork-like wrinkled joint at the base, spreading; *leaves* coriaceous, a span's length, opposite, perfectly entire, ovate oblong, lengthened sharp, smooth on both sides, with obsolete alternate nerves, shortly footstalked, the lower ones gradually smaller, the lowest ovate, subcordate at the base; *petiolets* short, thick, wrinkled; *flowers* within a common calyx, aggregated into a roundish head or fascicle, very beautiful, of the size of a fist; *fascicles* solitary, alternate, distant, sessile, subaxillary; *calyx common* imbricate, leaflets or bractes ovate, rather sharp, submembranaceous concave, rather lax, smooth, about two thumbs breadth long, red: each including single, or even two or three flowers; deciduous; the exterior rounded; the interior smaller, gradually linear; *perianth. proper* cylindric, tubulate, above rather enlarged, red, villose, bifid; with the divisions ovate, sharpish, subequal, erect; *corol. universal* uniform, blood-red; *proper* double; *exterior* infundibuliform, longer than calyx: tube cylindric, subangulate, narrowed downwards, sub-

coriaceous, permanent; border five-cleft, (often four-cleft): divisions lanceolate, obtuse, erect, unequal, one twice the breadth of the others, deciduous; *interior* pentapetalous, petals ovate lanceolate, obtuse, broadish, erect, nearly twice the length of the outer corol: claws subulate, inserted into the margin of the tube of the outer corol; *stamens. filaments* constantly eleven, filiform, very long, i. e. twice the length of the corol, erect, subcurvate, equal, beneath coalescing into an entire tube, opening in front, surrounding the germ, growing to the margin of the tube of the outer corol, then split into filaments equal at the base; *anthers* ovate, incumbent; *pistil. germ* superior, footstalked from the tube of the outer corol: the footstalk growing to the tube, cyindric, downy; *style* filiform, length of stamens, inflected; *stigma* simple; *pericarp. legume* oblong, compressed, narrowed about the dissepiment, common bilocular; *dissepiment* membranaceous; *seeds* solitary, ovate, compressed, somewhat wrinkled, covered with fungous fibres.

*XXIII. Fatal Effect of Lightning, in a Letter from the Rev. S. Kirkshaw, D. D., of Leeds. p. 177.*

On the 29th of Sept. last, (1772), about 2 o'clock in the morning, were 3 remarkably loud claps of thunder, attended with proportionable lightning. Mr. Thomas Heartly, formerly wine-merchant of Leeds, but lately retired from business to Harrowgate, lived there in a hired house, the 2d northward from the queen's head. While he was in bed with his wife, she was awaked from sleep by the thunder, and went to the window; but not being afraid, she went to bed again, and fell asleep. About 5 she awaked; and, not perceiving her husband to breathe, though warm, endeavoured to awake him—in vain! She quickly sent for Mr. Hutchinson, a considerable apothecary at Knaresborough, who, on sight of Mr. Heartly, and after some experiments, declared him dead, though still very warm. At her request however, he opened a vein; and Mr. Heartly bled freely, insomuch that the blood did not cease to ooze out of the orifice till the body was put in the coffin, which was on Thursday evening, the 1st instant, viz. October, and it was not even then cold. His hair, which he wore, was considerably burnt, or singed on the right side of his head, which was uppermost, as he lay then on his left side, and the inside of his night-cap, on the same side, was singed or browned, though no where on the outside marked at all. Within the cap was found a splinter from the bed post next to his head, which post was torn and split into many splinters or shivers, from the top to the bottom, though a strong oaken post, and almost new. No wound, or mark of any sort, was discovered on any part of his body; but the lower part of his right cheek was swelled, and much hardened.

In the chamber where this happened, there was a small chimney to the north,

made up, but not quite close, by a chimney board, on which could not be discovered any mark or hole, or other indication of the lightning passing that way. Between that chimney and the west end of the room, stands the bed, in the n. w. corner of the room, close to the west and north walls; the deceased lay next the west wall, with his head near the head bed post, in the n. w. corner abovesaid. There is only one window in the room, full east, consisting of 3 pretty large lights, separated by two stone mullions, each light supported by 6 strong iron bars across it, parallel to the floor, and the intermediate one, rather more than one half of it, made into a casement, the frame of which is of iron, and the surrounding frame of the same. In the southermost light, which had 3 squares of glass in breadth, 2 of the lowest squares were perforated in or near the middle, about an inch square; but as some small parts of the glass were gone, he could only guess at the size of the holes, nor could distinctly estimate the shape of them, nor form the slightest conjecture, whether the lightning had made its ingress or egress through both, or either of them. The intermediate square of glass left perfectly sound. There was no other iron about the window, except the abovementioned: but the curtain rods of the bed, which stood about 10 feet from the window, were iron, stronger (larger) somewhat than usual.

Mrs. Heartly lay on Mr. Heartly's left hand, when the thunder was, and felt not the least stroke from the lightning, or perceived any effects from it, except that her right arm, she found, when she awoke, was stunned and benumbed, and a little painful, which continued for a few days, but is now quite well. Dr. K. took notice of a pump, which stood about 10 or 11 feet from the house, in nearly a right line from the window abovementioned, the handle of which is all of iron, very thick and long, and a strong iron ball for a head to it.

*XXIV. On the Increase of Population in Anglesey. By Paul Panton, Esq., of Plaswryn, in Anglesey. p. 180.*

I wished to have sent you a fuller account of the state of the population in this island; but so little care has been taken to preserve the parish registers, that scarcely any that are ancient are to be met with. There is great reason to make the pleasing conclusion, that we become more healthy, and increase in population. Heretofore the inhabitants of this island lived chiefly on fish, with which, especially herrings, these coasts were abundantly furnished. Salted herrings were their principal food. This rambling fish, the herring, having left us, our islanders have neglected pursuing other branches of the fishery, and have betaken themselves more to agriculture. The potatoe plant was not cultivated in any great quantities here until of late years; but, since the failure of our herring fishery, it has made great part of the food of the inhabitants. Perhaps the want of the one, and the increased consumption of the other, may be among

the causes that have contributed to the better health of our people. The increase in population in Llanduvnan and Pentraeth parishes, has not been owing to mines, or any new advantage introduced. The inhabitants are wholly employed in husbandry.

The numbers baptized and buried, in 5 parishes, for several periods, are as below:

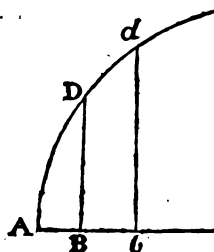
		Baptized.	Buried.
Llansadurn Parish	{ from 1590 to 1597.....	34.....	30
	{ from 1620 to 1627.....	34.....	33
	{ from 1750 to 1757.....	63.....	50
	{ from 1764 to 1771.....	69.....	68
Pentraeth Parish	{ from 1604 to 1671.....	69.....	188
	{ from 1672 to 1779.....	102.....	106
	{ from 1740 to 1747.....	100.....	85
	{ from 1764 to 1771.....	149.....	80
Llanwair yn Geornwy Parish	{ from 1732 to 1739.....	68.....	67
	{ from 1764 to 1771.....	101.....	77
Beaumaris Parish	{ from 1676 to 1683.....	135.....	174
	{ from 1710 to 1717.....	236.....	212
	{ from 1764 to 1771.....	328.....	249
Llanddyfnan Parish	{ from 1547 to 1554.....	36.....	39
	{ from 1620 to 1627.....	44.....	67
	{ from 1750 to 1757.....	111.....	46
	{ from 1764 to 1771.....	154.....	108

*XXV. A Letter to the Rev. N. Maskelyne, F. R. S., from Mr. Bailly,\* of the Royal Acad. of Sciences at Paris: Containing a Proposal of some new Methods of improving the Theory of Jupiter's Satellites. Translated from the French. To which are subjoined Notes on the same by the Rev. Samuel Horsley. p. 185.*

Sir, Though I have not the honour of being personally known to you, I flatter myself, you will excuse the liberty I have taken, of communicating to you

\* This very respectable astronomer, John Sylvain Bailly, was born at Paris in 1736, and was cut off in 1793, at 57 years of age. Early in life he showed a strong attachment to the sciences, and while very young communicated some valuable memoirs to the Royal Acad., of which he was elected a member in 1764. In 1768 he published the Eloge of Leibnitz, for which he had the honour of a gold medal from the academy of Berlin. This was followed by the Eloges of Corneille and Lacaille, which, with the former, were collected together. In 1775 appeared the first volume of his great and celebrated work, the History of Astronomy, the 4th and last volume of which came out in 1779. Besides these principal works, he published several astronomical observations and historical disquisitions. Mr. B. entered warmly into the convulsions of his native country, at the commencement of the revolution, and filled the critical office of president of the first national assembly. On July 14, 1789, he was also chosen mayor of Paris; but soon lost his popularity, owing to his enforcing obedience to the laws, and to the liberal sentiments he expressed for the royal family. In consequence he resigned his office in 1791, and retired again to the calm pursuit of the celestial sciences. But, in the sanguinary period that followed, he was apprehended, and after a summary process, condemned to be guillotined; which he suffered with firmness, the 12th of November, 1793.

two methods, of my invention, for perfecting the theory of Jupiter's satellites. The former of these methods serves to measure their diameters, and the latter is intended to make the observations comparable with each other, though made in different places, and with different instruments. You know, that the observations of the eclipses of the 3d and 4th satellites, made by different observers, vary from each other 3, 4, and 5 minutes, and sometimes more; and that there is even a pretty sensible difference in those of the 2d. In the 38th page of the preface to my Essay on the Theory of the Satellites, which has been presented to the R. S., I mentioned the inequality discovered by Mr. de Fouchy, and I suggested, that the perfecting of this theory might perhaps depend on the quantity of this inequality, which Mr. de Fouchy has not determined, not having been at leisure to resume the subject, since the year 1732. The segment of the disc which is not eclipsed, when the satellite disappears, must vary in the proportion of the squares of the distances of Jupiter from the sun, and from the earth. This is what a little reflection will make evident to every one, and this is the first cause of the inequality. Since Mr. de Fouchy's observation, it has been discovered, that the light of the satellite also decreases, in proportion to the proximity of Jupiter's disc; the brightness of the planet weakens that of the satellite, and, for this reason, the eclipses, which happen too near the opposition [of Jupiter to the sun], are considered as defective. Besides, the light of Jupiter, as well as that of his satellites, is different, in his different elevations above the horizon: when the planet is low, more rays of light are lost, in their passage through a thicker atmosphere; and whenever the light is less, the segment, which is not eclipsed when the satellite disappears, and which I call the insensible segment, increases, and occasions another inequality in the moment of the eclipses; lastly, the power of the telescopes, or their aperture, which, according as it is greater or less, give more or less light, contributes to the variation of this segment. Here then are 4 causes of inequality, which I reduce to one principle, and the following is the scope of my researches. When the satellite disappears, there is certainly a segment of its disc which remains uneclipsed; the magnitude of this varies, on account of the 4 causes just mentioned; thence it follows, that if in one eclipse the segment is  $ABD$ , and in another  $Abd$ , when the satellite disappears in the 2d eclipse, it will have got less into the shade, by a part of its diameter  $Bb$ : which part  $Bb$ , therefore, must be the value of the equation between the two eclipses. Now, if we call  $Ab$ ,  $a$ ;  $AB$ ,  $b$ , the radius of the disc of the satellite  $r$ , the semidiameter of the shadow, taken from the tables,  $R$ , and the total duration of the eclipse  $d$ , the time taken up in going over  $Bb$ , or the equation ( $^a$ ), will be  $\frac{2\pi r(a-b)}{d}$ , which





contains 3 unknown quantities, viz. the versed sines  $a$  and  $b$ , of the two invisible segments, and the semidiameter of the satellite's disc: for you know, sir, that there is nothing to be depended on, in all that has been done on the diameters of the satellites by Cassini, Whiston, and Maraldi. The following is the way which I have taken, to determine these unknown quantities. I observe, first of all, that 2 of them,  $a$  and  $b$ , are reducible to one; because, as you will see presently, the 2 segments are always in a known proportion [to the whole disc of the satellite, as well as to each other]; and consequently the proportion of their versed sines  $ab$ ,  $AB$ , may be obtained, either by calculation, or by a table made for the purpose. In order to discover it, considering that when the satellite disappears, it is from the diminution of its light, I conceived, that one might contrive to imitate, at any time, what happens in the eclipses, by diminishing the light. I have an acroamatic telescope of 5 feet length, and 24 lines aperture. I made some diaphragms of pasteboard, which I could apply on the outside of my object glass, the openings of which lessened, by half lines successively, from 24 lines down to 3. In fine weather, I applied these successively to my object glass, and endeavoured to find out, whether, by trying from the greatest to the less, some one of them could not be found, that would make the satellite disappear. My success in this gave me great satisfaction. One day, for instance, the 3d satellite disappeared, when the opening was reduced to 3 lines, and the 1st, when it was reduced to 6 only; and as, in the telescopes, the quantity of light is in the proportion of the squares of the apertures, I concluded, that the 64th part of the light of the 3d satellite, and the 16th part of the 1st, were insensible; whence it follows, that if, at the instant of an eclipse of the 1st satellite, the 16th part of its light is insensible, the invisible segment  $ABD$  will be likewise a 16th part of the disc; and thence it will be easy to compute the versed sine  $AB$ . In these first observations, I took care to chuse the time when the satellite was at its greatest elongation; for the insensible part increases prodigiously, and sometimes amounts to a 3d of the disc, when the satellite is very near the edge of Jupiter. This variation is much larger than that which takes place in consequence of the distance of Jupiter from the opposition to the sun, and contrary to it. As it is scarcely possible to estimate the law of the variations of this segment, occasioned by the proximity of Jupiter's disc, I judged that they ought to be determined by observation. Accordingly, I followed the satellite from the edge of Jupiter's disc, to the furthest limit of its eclipses, that is, with respect to the 1st, to the distance of 2 semidiameters of Jupiter. Having thus several points by observation, I got the rest by interpolation; and made a table of the variations of the invisible segment, which depend on the distance from the edge of Jupiter; a similar table I likewise made for each of the first 3 satellites; but have not yet been able to make sufficient observations on

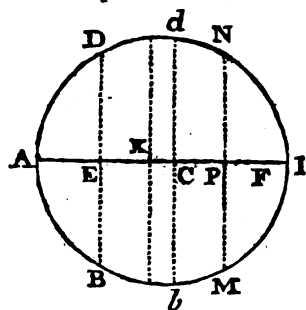
the 4th. These tables are contained in a long paper of mine, which will be published in the volume of our academy for 1771; but, if you please, I will send them to you. These segments being known, it is clear that, besides their variations occasioned by the distance of the satellite from the edge of Jupiter, they will be liable to others. First, in consequence of the change of Jupiter's distances, both from the sun and from the earth. On this account, the magnitudes of these segments being known, for a particular epoch,\* those known magnitudes must be multiplied by  $\frac{m^2 n^2}{p^2 q^2}$ , to determine the magnitude of the segment at any other time. In which expression,  $p$  and  $q$  denote the distances of Jupiter from the sun and from the earth respectively, at the given epoch, and  $m$  and  $n$  the distances, at the other time, for which the value of the invisible segment is required. 2dly. There will be other variations, depending on Jupiter's height above the horizon. The segments which I have observed, have all been reduced to the constant height of  $15^\circ$ . Mr. Bouguer, in his optics, has given a table of the degrees of light of the planets, at their different elevations above the horizon, which, from my own observations, I have found to be very exact, and useful for the present purpose. Now, as the segments are in the inverse ratio of the numbers of this table, putting  $g$  for the number corresponding to the elevation of  $15^\circ$ , and  $h$  for the number corresponding to any other elevation, the segments must be multiplied by  $\frac{g}{h}$ . 3dly. These segments will yet be subject to another variation, depending on the aperture of the telescope. It is certain that a larger aperture giving more light, the insensible part of the disc must be smaller; and it seems demonstrable by theory, that this insensible part, or the invisible segments, must be inversely as the squares of the apertures. I resolved however to assure myself of this by experiment. For this purpose, I carried my telescope to Mr. Messier's observatory, who has one of Dollond's telescopes, of  $3\frac{1}{4}$  feet length, and 40 lines aperture. On the 20th of August 1771, he saw the 2d satellite disappear in his telescope, through an aperture of 3 lines. The same satellite disappeared in mine, when the aperture was reduced to the same quantity of 3 lines, and not before. We changed instruments, and, repeating the experiment, found the same effect. Now, the insensible part was  $\frac{1}{16}$  of the disc, in Mr. Messier's instrument, and  $\frac{1}{25}$  in mine. These portions therefore, in these telescopes, were in the inverse ratio of the squares of the apertures. Consequently, in order to determine the segments for an aperture of any number of lines  $k$ , the segments of my table, which are all calculated for an aperture of 24 lines, must be multiplied by  $\frac{576}{k^2}$ . Hence, to compute the invisible seg-

\* Known, by the author's tables, for any distance of the satellite from the edge of Jupiter, at the particular epoch to which the tables are adapted.—Orig.

ment for any particular eclipse, the actual distance of the satellite from the edge of Jupiter being known, look for the quantity of the invisible segment which answers to that distance, in my table, and multiply this quantity by

$\frac{m^2 n^2}{p^2 q^2} \times \frac{g}{h} \times \frac{576}{k^2}$ . If two different observations, made in the same place, or rather two observations made in different places, by different observers, are to be compared, the invisible segment must be determined, such as it was for each observer;  $AB$  and  $ab$ , the versed sines of these segments must be computed, and in the expression  $\frac{2ar(a-b)}{d}$ , the only remaining unknown quantity will be  $r$ .

The following is the method I have hit upon for determining it. I considered, that by trying different diaphragms successively, some few minutes before an immersion, it would be easy to find out the particular size which would make the satellite disappear; and that the proportion of the invisible segment to the whole disc of the satellite, for that instant, would by that means be determined. Suppose then that I have found this diaphragm: my next step is, to cover the object-glass of my telescope with a diaphragm somewhat larger, which suffers me just to perceive the satellite, but so weak and small, that the least further diminution of its light must render it invisible. I wait till it actually disappears; I write down this time, then take away the diaphragm, and the number of seconds which pass between this first disappearance and the true immersion, giving me a great part of the diameter, I easily compute the whole. The following is an example of my method. On the 26th of June 1771, there was an immersion of the 3d satellite, at 56<sup>m</sup> after 9 in the evening. I found the diaphragm which made the satellite disappear, to be of 12 lines. I then fitted my glass with a diaphragm of 17 lines; I might have taken one much smaller: presently the satellite disappears. But removing the diaphragm, I see the satellite again, very distinctly, for 2<sup>m</sup> 18<sup>s</sup>; after which the true immersion followed. Now this is my calculation. The aperture of the diaphragm, which made the satellite disappear, being 12 lines by observation, the invisible segment at the instant of the eclipse, must have been a quarter of the disc. Let  $ABD$  be this quarter. I



I know that, at the instant of the immersion, the satellite had entered the shade, by the whole part  $EF$  of its diameter. I say then, if on an aperture of 24 lines, the part  $ABD$  is insensible, the insensible part, on an aperture of 17 lines, will be larger than  $ABD$ , in the ratio of the square of 24 to the square of 17. Saying, then as  $17^2 : 24^2 :: 0.25000 :: x$ ,  $x$  comes out = 0.49827, or near half the disc, represented by unity;

thence I see that, at the instant of the first disappearance, the satellite had not gone in farther than  $K$ . Putting the radius  $AC = 1$ , the versed sines  $AE$ ,  $AK$

will be = 0.59602 and 0.99884; consequently  $EK = 0.40282$ . Substitute this value of  $EK$  instead of  $a - b$ , in the expression

$\frac{2R \times r(a-b)}{d}$ , and you will have  $\frac{2Rr \times 0.40282}{d} = 2^m 18^s$ , and  $r = 5^m 23^s$  of time, will be the semidiameter of the satellite, or the whole diameter will be  $22' 34''$  (of the satellite's orbit, considered as a circle, or would be seen under an angle of this quantity, from Jupiter's centre). In this observation, as I have already said, I used a diaphragm with too great an opening ( $c$ ), for the first disappearance. Take then a 2d observation. On the first of August 1771, there was an emersion of the 3d satellite, about  $15^m$  after 9. I marked the instant of this emersion; then I furnished my telescope with a diaphragm of 8 lines. The satellite disappeared, and did not begin to appear again till at the end of  $6^m 24^s$ . Some minutes after, when it was quite come out of the shadow, I measured the diaphragm, which would make it disappear, and found it of 7 lines. These 7 lines give a segment  $ABD$  of 0.08507. Then saying  $8^s : 24^s :: 0.08507 : x$ ;  $x$ , or the segment  $ANM$ , comes out = 0.76562,  $AE = 0.27994$ ,  $AP = 1.43098$ , and  $EP = 1.15104$ . Therefore  $\frac{2Rr \times 1.15104}{d} = 6^m 24^s$ . From this equation  $r$  comes out  $5^m 20^s$ , and the whole diameter  $22' 22''$ , (in parts of the circular orbit). These two conclusions agree so perfectly well with each other, that, if I am not too fond of my own work, I may venture to say, the method I have invented may be carried to great exactness. I hope you will have the goodness to give me your opinion of it, which I have the greatest respect for, and will be very useful to me, especially if you have leisure to repeat the observations. I shall take the liberty to subjoin a few hints, on the manner of making them, at the end of this letter. The diameter of the 1st satellite I have determined by 3 observations as follows:

$$\begin{array}{l} \text{An Immersion} \\ \text{Emersion} \\ \text{Emersion} \end{array} \left\{ \begin{array}{l} \text{June 30} \\ \text{Aug. 1} \\ \text{Sept. 2} \end{array} \right\} \text{ gave } \left\{ \begin{array}{l} 7^m \ 17^s \\ 7 \quad 3 \\ 7 \quad 1 \end{array} \right\} \text{ in time or } \left\{ \begin{array}{l} 1^s \ 1' \ 45'' \\ 0 \ 59 \ 46 \\ 0 \ 59 \ 29 \end{array} \right\} \begin{array}{l} \text{as} \\ \text{seen} \\ \text{from} \end{array} \left. \begin{array}{l} \\ \\ \end{array} \right\} 24.$$

You see, sir, that this agreement is likewise very satisfactory. Mr. Messier took part in these observations, and found the application of them very easy. He himself observed the diameter of the 2d satellite, by the emersion of the 30th of August; but it emerged at so small a distance from the 1st, that this circumstance may have vitiated the observation; the diameter of this satellite must therefore be verified by fresh observations. However, the result of Mr. Messier's makes it  $7^m 2^s$  in time, or  $29' 42''$  seen from Jupiter; and a former observation of mine, of the 11th of July, gave the same quantity precisely. Thus, by means of the tables given in my paper, which I had the honour of mentioning to you, it will be possible to compute the invisible segment, for all the observations which have been hitherto made, and the diameter of the satellite being

likewise ascertained, to reduce the instant of the observed eclipse to that of the passage of the centre over the edge of the shadow, which will be a fixed term for all the observations, and all the observers, who but seldom agree in the observation of the same eclipse. I confess that the transparency of the air is not always the same, and that a greater or less degree of transparency will make the segments smaller or larger, and consequently affect the observation. The inequality of sight may likewise occasion some error; for though it might be possible to settle the general effect of the difference of sight of different observers, the sight of the same person is not constantly the same, and even independently of the change produced by age, may not have the same strength at all times. But by the method I propose, all these inconveniencies will be remedied, in future observations, with little trouble. Every observer is to furnish himself with several diaphragms of pasteboard, gradually diminishing by half-lines, to be applied to the object-glass externally, and some minutes before an immersion, or after an emersion, he is to determine which of them intercepts from him the sight of the satellite. Having found this, and knowing also the diameter of the satellite, he will reduce, by the process of calculation already explained, the observed instant of the eclipse, to that of the passage of the centre; which is the same, as I said before, for all the observers in the world. You see, sir, what advantages would arise, from this agreement, for the theory of the satellites, and the precision of the terrestrial longitudes. This method takes in every thing; the difference of glasses, that of sights, the greater or less transparency of the atmosphere, &c. Observation gives the segment greater or less, in proportion to the combined influence of all these causes. The principal advantage of this method, which requires only a very simple calculation, is, that it depends on no hypothesis. It enables us to measure immediately the light of the satellite, whether increased or diminished by all the causes above mentioned; to measure, I say, the real impression of that light upon the eye, whatever be the actual state of the organ. I must add, that I am sensible, the determination of the invisible segment, by means of the diaphragm, might be inconvenient to those, who make use of large telescopes for the eclipses of the satellites, were it not, that this observation may be equally well made with a smaller telescope, provided only, that it be sufficient to see and distinguish the 4 satellites; and after the diaphragm is determined by this smaller telescope, the larger one may be used for the observation of the eclipse. For these measures are easily transferred from one instrument to another, the invisible segments in different telescopes, being inversely as the squares of the apertures. For reflectors, I have a method of the same kind with the former, grounded at least on the same principles, by which I can determine their power, and compare them, both with each other, and with the refracting telescopes. I shall conclude with some hints concerning the observa-

tion of the diaphragm, for determining the invisible segment. To repeat these observations with judgment, it will be necessary to recollect the intention of them; which is, to measure what portion of the disc remains illumined, that is, what portion of the satellite's light continues, though unperceived, to be transmitted to the observer's eye, at the instant when the satellite disappears, on the brink of an eclipse. In lessening gradually the aperture of the glass, the observer should not begin with too small an opening; because the eye, not accustomed to the great obscurity which follows, might not see the satellite at all. As the opening is gradually contracted, the satellite seems to grow less. The observer sometimes loses sight of it for a moment; but if he continues to look attentively, he sees it again. The real disappearance is only to be concluded, when, on fixing with steady eyes, for about half a minute, on the place it occupied, it is seen no more; for if one persisted to observe it much longer, it might happen, that it might be seen to glimmer at times, and immediately disappear. I have always made it a rule, to consider the debilitation of the light, in this degree, as actual disparition, and it is necessary that observers should agree on this point, in order that their different estimations may be consistent. These fits of momentary glimmering and extinction are undoubtedly owing to the motion of the particles of the atmosphere. In the clearest weather, there are always particles of vapour floating in it, in vast abundance; according as these particles place themselves in the direction of the ray of light, or out of it, the light of the satellite is diminished or restored, and the satellite, in consequence, is either hid or rendered visible. This does not happen in eclipses, wherein a great part of the light is in reality extinguished. But in the case I am now speaking of, though the diminution of the aperture of the glass does indeed take away a great quantity of it, yet this quantity is always relative to the actual state of the atmosphere: (<sup>d</sup>): if that state changes, this quantity becomes alternately sensible or insensible, according as the light meets with more or less obstructions in its passage from the vapours. Another thing, which it will be necessary to point out, is, that the operation with the diaphragms, for determining the invisible segment, must be made and concluded before the satellite has touched the shadow. The proper time therefore for beginning this observation, will be determined by the time the diameter takes in entering, which, in the perpendicular ingress, or when Jupiter is in the nodes, is 7<sup>m</sup> for the first 2, and 11<sup>m</sup> for the 3d. The time of the oblique ingress is  $(7^m) \frac{2R}{d}$  for the first two, and  $(11^m) \frac{2R}{d}$  for the 3d; which, for this last, may in extreme cases amount to about 27<sup>m</sup> or 28<sup>m</sup>. It is proper to take 5<sup>m</sup> more; for the observations of the diaphragm will take up 2<sup>m</sup>, even when use has rendered them familiar, and the tables may be 2<sup>m</sup> or 3<sup>m</sup> behind. At present, it is sufficient to begin the observation 16<sup>m</sup> before an eclipse of the 3d satellite; but there are times, in which it would be neces-

sary to begin 20<sup>m</sup> or 30<sup>m</sup> before. It is essential not to begin too late, for fear of missing the observation; it is also essential not to begin too soon, because then the segment measured would be too small, as the satellite is continually either approaching to Jupiter, or receding from him. All this is hastily explained; but these matters are so familiar to you, that you cannot but understand me, and this letter is already too long. I am afraid it will tire you; but I am extremely desirous of having the exactness and utility of these two methods, the one for the measure of the diameters, the other, for making all the observations capable of mutual comparison, ascertained, by repeating the observation of the diaphragm in every eclipse. I cannot take a better way than to consult the several astronomers, who, like you, besides being deeply skilled in the theory, are the most celebrated observers. If they will adopt this method, it will be the best way of making it general, as others will follow it of course. I have communicated it to Mr. Messier, who proposes making use of it. Mr. Maraldi, who is gone to his house near Nice, has tried the observation of the diaphragm with success, with an achromatic telescope 34 feet long; and he would already have made use of it, but that it is impracticable with the telescope of 15 feet, which he uses for the eclipses of the satellites. I have written to him that he may observe the eclipse with his usual telescope, and the diaphragm with the achromatic; so that I make no doubt he will use this method as soon as Jupiter shall have come out of the sun's rays. These, Sir, are the things on which I wish to consult you, and have your advice. I shall be much flattered by your communicating this letter to the R. S., if you think it deserves attention.

*Notes on the Foregoing Paper. By the Rev. Samuel Horsley. p. 213.*

(\*)  $\frac{2Rr(a-b)}{d}$ . This formula is deduced from the following principles. 1st. That the motion of the satellite, in its orbit, is uniform, or at least may be considered as such, without sensible error, in the present investigation. 2. That the time which the semidiameter takes to enter the shadow, in any eclipse, is inversely as the whole time of the duration of the eclipse. 3. That the time which any given part of the semidiameter takes to enter the shadow, is to the time which the whole semidiameter takes to enter, as that part to the whole.

Now, let  $a$  and  $b$  denote the versed sines of the arcs  $Ad$ ,  $AD$  (in the first figure) respectively, the radius being unity. Let  $R$  denote the half-time of the duration of an eclipse, when Jupiter is in the node of the satellite's orbit,  $r$ , the time which the semidiameter takes to enter the shadow in such eclipses;  $d$ , the whole duration of an eclipse, happening when Jupiter is at any given distance from the node. Then will  $\frac{2Rr}{d}$  express the time which the semidiameter of the satellite will take to enter the shadow, in the eclipse whose duration is  $d$  (by 2<sup>d</sup>, because  $d:2R=r$ :  $\frac{2Rr}{d}$ ). And,  $\frac{2Rr}{d}$  being the time that the semidiameter takes to enter the shadow,  $\frac{2Rr(a-b)}{d}$  will be the time that the part  $ab$  takes to enter, by 3<sup>d</sup>.

It is to be observed, that, to compare two eclipses by this formula, it is necessary, that the planet should have been at the same distance from the node of the satellite's orbit, at the commencement of

both. For comparing eclipses otherwise circumstanced, a more general formula may easily be deduced from the same principles. If  $\Delta d$  be the insensible segment in one eclipse,  $\Delta D$  in another (vide the first figure),  $a$  the versed sine of the arc  $\Delta d$ ,  $b$ , of  $\Delta D$ , the radius being unity,  $d$  the whole duration of the first eclipse,  $\delta$  of the 2d, then  $\frac{2Rr}{d\delta} \times (\delta a - db)$  is what the author would call the equation, be-

tween the two, arising from the different magnitudes of the insensible segment, or the time by which the interval between the observed eclipses, differs from the interval between the real passage of the centre in each eclipse. This is a general formula, for all eclipses of the same satellite. If the planet's distance from the node has been the same in both, then  $\delta = d$ , and this formula changes into the author's. The more general one is here given, rather for the fuller explication of the theory, than for any necessity that there is to have recourse to it in practice. For, though the use of it may sometimes be convenient, eclipses of the same satellite may always be compared without it, when once the diameter of the satellite is known, and the magnitude of the insensible segment in each eclipse determined, by reducing the observed immersion or emersion to the true ingress or egress of the centre.

(*b*) The words printed in the Italic characters are designedly omitted in the translation, it being apprehended, that it is owing to some inadvertency, that they appear in the author's text. For unless they are expunged, the general description, here intended, of the author's method of determining the diameters of the satellites, will by no means agree with the examples of that method immediately subjoined. These words imply, that the author takes the instant of the disparition of the satellite, in the contracted aperture of the diaphragm, for the moment of the contact of the satellite's limb with the edge of the shadow, and makes that moment so determined, the basis of his calculations: reasoning as it should seem thus. 'When any part of the diameter of the satellite, however small, has entered the shadow, some part of the light, which the observer receives, through the aperture of the diaphragm, from the whole unshaded disc, will be intercepted. But that aperture is so small, that the light transmitted through it, from the whole unshaded disc, is but just sufficient to be sensible; and must therefore cease to be so, when it is in the smallest degree diminished, i. e. when the very smallest part imaginable of the disc is shaded. Therefore the moment of the disparition, in the aperture of the diaphragm, is the true commencement of the eclipse, or differs from it by less than any assignable difference.'

But it appears, from the examples given afterwards, that the author's calculations proceed on much safer principles. Having determined the portion of the disc, that is insensible on the whole given aperture of his telescope, he computes what larger portion will be insensible, on the smaller given aperture of his diaphragm. And then, by observing the two disparitions, the earlier one in the diaphragm, the other in the telescope with the object-glass uncovered, the last of which he calls the true immersion, he knows the time in which a given portion of the diameter enters the shadow, and consequently the time in which the whole enters; which determines the magnitude of the whole, in parts of the satellite's orbit, or its apparent magnitude to an observer of Jupiter's centre.

(*c*) The disadvantage of using too great an aperture is, that the part of the diameter obtained by observation, from which the whole is to be concluded, will be less than the same method of observation would give, with a more contracted aperture. For the larger the aperture of the diaphragm is which is applied to the object-glass, the less is the difference between that aperture and the whole aperture of the telescope; consequently the less is the difference between the segments, which are insensible in these apertures severally, and the less the portion of the diameter, which passes over the shadow's edge, between the two disparitions.

(*d*) In eclipses, when once the satellite has disappeared, or is become visible, the author says, we are not to expect those fits of glimmering and extinction, which he has described as taking place, when we observe the uneclipsed satellite with very contracted apertures. The reason is plainly this: in immersions, a part of the disc is still indeed enlightened, when the satellite disappears; and the quantity of light, transmitted from this part to the observer's eye, must be very different, in different states of



the air's transparency; and consequently the satellite, after having disappeared, might become visible again, by a sudden increase of the air's transparency in the tract of the satellite's light, provided the magnitude of the unshaded part remained, at the instant of the increased transparency, what it was when the satellite first disappeared. But as this is not the case, as the unshaded part is continually becoming less, the satellite cannot reappear, unless the increase of transparency be such, as to overbalance the diminution of light made by the progress of the eclipse. And the motion into the shade is so quick, that this can rarely, if ever, happen. By the like reasoning, fits of extinction are not to be expected, when once the satellite has shown itself in an emersion.

The author of these remarks does not imagine, that any apology is necessary for the liberty he has taken. He has the highest opinion of the merit of Mr. Bailly's invention; and this has excited him to contribute what he could to obviate objections, and to prevent mistakes.

*XXVI. A short Account of an Explosion of Air, in a Coal-pit, at Middleton, near Leeds in Yorkshire. In a Letter from Mr. W. Barnard, of Deptford.*  
p. 217.

I have at length procured from my father, a memorandum made by him on the spot, of the effects of foul air set on fire, which I have copied exactly as below:

“Being engaged in Middleton wood, the estate of Cha. Brandling, Esq. near Leeds in Yorkshire, in directing the falling and barking of a large quantity of timber bought of him in May 1758, I was witness of the following accident. Some miners, being to renew their operations on the shaft of a coal-pit, which, in a former year, had been sunk to the depth of 60 yards, in order to get through a stratum of very hard stone, thought proper to drill holes, and fill them with gunpowder. They afterwards from the top, threw down fire to blast the stone, which made a report little louder than that of a pistol; but the blaze setting the foul air on fire, produced an effect truly shocking. The whole wood was shaken, the works at the mouth of the pit were all blown to pieces, and the explosion was such as cannot be described. The vacuum in the air was so considerable, that oak trees of a load or more each, at a great distance from the pit's mouth, that before stood upright, stooped towards the pit very much, and must have fallen wholly down, had not the air been instantly replaced. The bark-pullers, at a quarter of a mile from the pit, were so alarmed by the shaking and explosion, that not one of them would have remained in the wood, had they attempted to blast it again. N. B. The trees in the whole circuit stooped towards the pit.”

*XXVII. Extract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, Rutland, 1772. By T. Barker, Esq.* p. 221.

This is a register of the highest, lowest, and mean state, of the barometer and thermometer, as also the quantity of rain, for each month of the year 1772. The whole depth of rain for the year was 28½ inches.

*XXVIII. Observations on the Lagopus, or Ptarmigan. By the Hon. Daines Barrington, V. P. R. S. p. 224.*

The many different specimens of lagopi, both in their winter and summer plumage, which have lately been presented to the R. S. from Hudson's Bay, enable us to correct many mistakes that have hitherto been made in the description of this bird; as well as the unnecessarily multiplying of the species of the tetrao genus. As M. de Buffon is the last ornithologist who has made any observations on this bird, it may not be improper to take notice of some of his supposed inaccuracies. The lagopus, of which he gives an engraving, is in its winter plumage; and the feet of the bird are consequently covered very thickly with feathers. M. de Buffon however, from not having examined the specimens of the lagopus with proper attention, says, that Aristotle could not have been acquainted with this bird, because the under parts of the claws are entirely covered with feathers; which circumstance is so very striking and peculiar, that it could not have escaped this father of natural history. If a winter specimen however of the lagopus, or ptarmigan, be accurately examined, it will be found, that no feathers grow precisely under the claws; though, by wrapping very thickly round them, they have very strongly that appearance: and, in a summer specimen, not only the feet, but even the legs, are rather bare of plumage. If Aristotle therefore had procured the bird in its summer dress, he could not have observed this very striking circumstance, which M. de Buffon relies on as so strongly characteristic.

The same difference between the plumage in summer and winter is experienced in each of the three species of tetrao, which have (according to one of Linnæus's subdivisions) feathered feet; and it is usually said with us, that they have in winter their snow boots. M. de Buffon therefore unjustly charges the author of the British Zoology, for supposing that this is a wise provision of Nature against the inclemency of the season, when he says, that the vrogallus minor, or our black cock, has not the same protection for its feet, though it buries itself under the snow, and, becoming torpid, equally wants such additional warmth. With regard to the torpidity of this bird, M. de Buffon relies on Linnæus's asserting, that sæpe sepelitur in nive; which by no means signifies that the bird is torpid, but only that it buries itself sometimes under the snow; as sheep do with us in the more rigorous seasons, when it lies very deep in the mountains. The black cock however is so far from being torpid in the winter, that it even approaches the habitation of man when distressed for food; and Mr. B. concludes, till he shall see a specimen which proves the contrary, that, like the other tetraos, whose feet are covered low with feathers, this part of the plumage becomes thicker in winter. M. de Buffon also seems to be mistaken in supposing, that the thick plumage round the feet is peculiar to the lagopus; as it is believed that

Linnæus's first division of this genus have, all of them, the same additional cloathing for the winter; nor is this extraordinary warmth confined merely to this genus, as the noble specimen of the large white owl, which has lately been presented to the R. S. from Hudson's Bay, is covered about the claws with a plumage of perhaps an equal thickness.

The next remarkable circumstance in this bird is, that the shafts of many of the wing-feathers are black; which M. de Buffon supposes to be only 6; whereas they are 8 in the specimens from Hudson's Bay; the last 2 are indeed of a fainter colour. M. de Buffon next says, that Brisson counts 18 feathers in the tail; and Willoughby 16; which he himself reduces to 14. It seems however that Willoughby's number is the more accurate; and, by examining the difference between the summer and winter specimens, the black feathers of the tail are found covered by 2 upper ones, which in summer are brown, and in winter white. Neither can Mr. B. discover, in any of the specimens, the 2 white feathers in the tail, according to Linnæus's description, *rectricibus nigris apice albis, intermediis albis*, as the 2 covering feathers before-mentioned cannot, with propriety, be termed *intermedii*, nor are they white in the summer but brown; so that Linnæus makes a circumstance, which varies with the season, to be a permanent characteristic of the bird.

M. de Buffon next supposes, that Willoughby and Frisch speak of different birds under the name of lagopus; because the first says that the feet are covered with soft, and the latter, with harsh and bristly feathers. The remarks however of these ornithologists are easily reconciled, for, if the finger is drawn according to the course of the feathers, they feel soft; and if in the contrary direction, harsh and bristly. The difference also between Belon, Gesner, and Linnæus, with regard to the call of this bird, is as easily accounted for; because most male birds differ from the female in this respect, and sometimes the young birds from those which are full-grown.

This naturally brings Mr. B. to show, that M. de Buffon (who has great merit in other parts of his Natural History, by not unnecessarily multiplying the species of animals) has, in this kind of tetrao, considered as 2 species what, when properly examined, will turn out to be only the lagopus, or ptarmigan. His chief reason for considering the lagopus of Hudson's Bay as being distinct from the ptarmigan, arises from his asserting, that Mr. Edwards, in his description of that bird, says it is twice as large. Mr. Edwards however only considers the size of the Hudson's Bay lagopus as between that of a pheasant and a partridge, in which he is very accurate; the bird is not only evidently so to the eye, but weighs 3 ounces more than a common partridge.\* M. de Buffon likewise

\* The partridge, when full grown, weighs 13 ounces, and the ptarmigan 16.—Orig.

seems to make an unnecessary species of tetrao, under the name of le petit tetras, a plumage variable; as his principal argument for this opinion is, that they are not found on the mountains, as the lagopi are. Now it is very clear, from the name given in the catalogue from Hudson's Bay to this bird, of the willow partridge, that it lives entirely in that part of the world on the plains; nor are there, it is believed, any very high mountains in the neighbourhood of our forts.

When M. de Buffon therefore conceives, that the lagopus is always endeavouring to find out snow and ice, and that it carefully avoids the glare of the sun; it should seem that the observation is by no means generally true; because, though the rigour of a Hudson's Bay winter is great, yet the summer is very pleasant, and the snow soon disappears, without which M. de Buffon imagines that the bird cannot exist; though his 9th plate represents the ptarmigan, in his winter dress, surrounded with trees and plants in most luxuriant foliage and vegetation. A new observation is, that the claws are scooped off at the end exactly like a writing pen, only wanting the slit; which circumstance may likewise be seen in the claws of our common grouse, or heath-game, though the resemblance is not quite so strong as in the ptarmigan.

Mr. B. concludes with copying, from the catalogue transmitted with the specimens from Hudson's Bay, what further relates to the lagopus; which, as before observed, is there called a willow-partridge.\* "The willow-partridges gather together in large flocks in the beginning of October, harbouring amongst the willows, the tops of which are their principal food; they then change to their winter dress. They change again in March, and have their complete summer dress by the latter end of June. They make their nest in the ground in dry ridges; and are so plentiful, that 10000 have been killed in the three forts in one winter."

*XXIX. On the Effects of Lightning at Steeple Ashton and Holt, Wilts, June 20, 1772, extracted from several Letters, communicated by Edw. King, Esq., F.R.S. p. 231.*

L. Eliot, vicar of Steeple Ashton, in Wiltshire, writes, that on the 20th of June, 1772, between 12 and 1 o'clock in the afternoon, a violent storm of thunder and lightning happened at Steeple Ashton, in Wiltshire. During the storm, a woman in the village saw a large quantity of lightning come out of a cloud, part of which is supposed to have fallen on the top of the north chimney

\* It is not at all extraordinary, that it should there be considered as a partridge, because the white partridge is the name given to this bird by the old ornithologists, who have very naturally considered edible birds nearly of the same size as partridges, when they have short tails, and as pheasants, when they have long ones.—Orig.

of the vicarage house, attracted probably by an iron hoop that went round the chimney, and by some iron bars placed within it, that formerly made part of an apparatus to prevent its smoking. That the lightning fell on these iron bars is very probable, because the colour of two of them that were contiguous was changed, 9 or 10 inches in length, to a dark blue, like that of a watch spring, no uncommon effect of electrical fire.

In the north parlour, to which this chimney belonged, were the Rev. Mr. Wainhouse, of Steeple Ashton, and the Rev. Mr. Pitcairn, of Trowbridge, the former standing, and the latter sitting in a great chair, with his back to the fireplace, near the wire of a bell. In the south parlour, separated from the other by a hall, were a maid servant and a painter; in the kitchen another maid servant; in the coal-house, 4 or 5 yards from the house, a man servant; near the barn, about 50 or 60 yards from the house, another man servant. When the lightning fell on the house, the man servant near the barn heard a very loud noise, equal, he supposes, to the sound of 20 cannons fired at once, and would have fallen to the ground, if he had not caught hold of something to support himself. The other man servant in the coal-house was struck backward, and felt something, as he describes it, like a stream of warm water poured on the middle of his body, which, if it was not the electric fluid itself, was the heated air expanding itself with violence after the explosion. The maid in the kitchen heard a great noise, but received no shock. The other maid servant, who was standing near the middle of the south parlour, suffered likewise no shock, being only terrified exceedingly with the explosion, and the sparks of fire, which she saw on all sides of her; but the painter, who was in the same room, painting near the chimney and the bell wire, was struck on the left side of his body that was next the wire, from his head to his waist; he felt in particular a severe shock, like the electrical one, in his left wrist, which was marked all round with blue and yellow intermixed; a splinter from the wooden case, that covered the bell wire, struck through his glove, and wounded his hand, and he was stunned for some time. Immediately after the woman had seen the lightning come from the cloud, as above-mentioned, some persons in the village, besides those in or near the vicarage house, were thrown to the ground.

The following is the account, which Mr. Wainhouse and Mr. Pitcairn give, of what happened in the north parlour in which they were. As they were conversing about a loud clap of thunder that had just happened, they saw on a sudden a ball of fire between them, on a level with the face of the former, and about a foot from it. They describe it to have been of the size of a sixpenny loaf, and surrounded with a dark smoke; that it burst with an exceedingly loud noise, like the firing of many cannons at once; that the room was instantly filled with the thickest smoke; and that they perceived a most disagreeable smell,

resembling that of sulphur, vitriol, and other minerals in fusion; insomuch that Mr. Pitcairn thought himself in danger of suffocation. Mr. Wainhouse providentially received no hurt, except a slight scratch in his face from the broken glass that was flying about the room, a kind of stupefaction for some time, and a continued noise in his ears, which noise, the effect of the explosion, happened likewise to Mr. Pitcairn, and others in the house.

The lightning fell on Mr. Pitcairn's right shoulder, made a hole in his coat, about a quarter of an inch in diameter, went under his arm in one line to his breast, thence descended down the lower parts of his body in two irregular lines, about half an inch broad, attracted probably by his watch, the glass of which it shivered in small pieces, and meeting perhaps with a little resistance from it, spread itself round his body, and produced the sensation of a cord, tied close about his waist. A violent pain in his loins immediately followed; and thence to his extremities there seemed to be a total stoppage of circulation, all sensation being lost, and his legs and feet resembling in colour and appearance those of a person actually dead. Besides shivering the glass of his watch, the lightning melted a little of the silver of it, and a small part also of half a crown in his pocket. When it came to the middle of his thigh, it left an impression of a blackish colour, resembling the branch of a tree, which in a few days disappeared; but the lines on his body are still visible, and are of a dark blue, intermixed irregularly with a deep yellow. From the middle of his thigh the lightning changed its direction again, and went down the under side of it to the calf of his leg, and so to his shoe, which was split into several pieces in so remarkable a manner, as justly to claim the inspection of the curious. As soon as Mr. Pitcairn was struck, he sunk in his chair, but was not stunned; his face was blackened, and the features of it distorted. His body was burned in several places, small holes were made in different parts of his clothes, and he lost in some measure the use of his legs for 2 or 3 days; but by proper care he soon recovered, except a weakness and numbness in his right leg, which still remains. What is remarkable, Mr. Pitcairn remembers very well to have seen the ball of fire in the room for a short time, a second or two, after he found himself struck with the lightning. Extraordinary as this circumstance may appear, it may be proper to take notice, that it is entirely agreeable to an observation of the learned and ingenious Dr. Franklin, quoted below.\*

The effects of the lightning on the building and furniture were as follow. The north chimney was thrown down, the roof and ceiling near it beat in; large

\* In every stroke of lightning, I am of opinion that the stream of the electric fluid, &c. will go considerably out of a direct course for the sake of the assistance of good conductors; and that in this course it is actually moving, though silently and imperceptibly, before the explosion, in and among the conductors, &c. Franklin's Experiments and Observations on Electricity, edit. 4, p. 124.—Orig.

stones were forced out of the walls, some were driven to a considerable distance, one in particular to about 200 feet. The glass of the windows in the north parlour and the chamber over it was forced outwards, except in the casements, which were open, and in which not a pane of glass was broken. The case of a clock in the same parlour fell forwards, and was beaten to pieces; a looking glass over the chimney was thrown on the floor and broken, some of the quicksilver was melted, as was likewise some of the lead belonging to the windows. A bureau, that was locked, was opened; as was also the parlour door, inwards, probably by the external air rushing in to restore the equilibrium. Some bedding in one of the chambers was fired, but the fire was extinguished of itself, or by the rain that fell during the storm, before it was discovered. Several splinters were torn out of a hogshead full of beer, but the cask was not materially damaged, nor the beer spilt. The iron bell wire in both the parlours and the hall was reduced to smoke and entirely dissipated, excepting in those parts where it was twisted, and double, and also the wire springs contiguous to the bell, which the lightning left undamaged, as well as the brass handles and bell itself. The ceiling and wall on each side, where the wire went, was stained irregularly, a foot or more in breadth, with a dark blue intermixed with a deep yellow. It is worth observing, that this iron bell wire was very small, considerably less than a common knitting needle; but though it was itself destroyed, yet it seems to have served as a conductor to the lightning, and to have prevented worse effects than happened. For when the lightning had run along, and consumed all the single wire, and had reached that which was twisted and double in the south parlour, contiguous to the brass handle, which the bell used to be rung with, it made a hole in the wall of 5 or 6 inches in diameter, being attracted probably by an iron stove on the other side in the kitchen chimney, where meeting with several large conductors, handirons, poker, tongs, &c. it seems to have been conveyed into the ground. This appears probable, because the progress of it below stairs could not be traced beyond this hole, which it made in the wall. In the chamber over the kitchen, a small piece of wood was indeed struck out of a bed post, and the glass of half a window was driven outwards; but this does not seem to have been the immediate effect of the lightning, but of the shake from the explosion.

Whether Steeple Ashton is from its situation particularly exposed to thunder storms, is uncertain. It may however be proper to mention, that in the year 1670, July the 25th, a violent storm of thunder and lightning damaged the church steeple, which was 93 feet high; and on the 15th of October in the same year, another thunder storm threw it entirely down, and killed 2 of the workmen, who were repairing it.

P. S. Mr. Field, a painter of Trowbridge, during the storm, observed a ball

of fire vibrate forward and backward in the air over some part of Steeple Ashton, and at last dart down perpendicularly; which in all probability was the ball of fire that Mr. Wainhouse and Mr. Pitcairn saw in the north parlour of the vicarage house. Another circumstance is as follows: after the explosion of the ball of fire in the north parlour, Mr. Pitcairn observed a great quantity of fire of different colours vibrating in the room forwards and backwards with a most extraordinary swift motion.

*To the Rev. Mr. Eliot, from Mr. William Paradise.*

During the storm a person in this place (Holt) saw a body of fire moving towards a house that is next to mine, though at some distance from it; attracted probably by a large iron bar of 10 or 12 feet long, fixed horizontally to support a high chimney. This body of fire changed its direction, and fell on my house, forced a brick out of the chimney, near that part of it to which the iron bar was fastened, and went through the house to an outer door on the opposite side, which happened to be open; there it burst with a loud noise, like the firing of cannons, and filled the room where I was with smoke and the smell of sulphur. I was fortunately 3 or 4 feet out of the line in which it moved. I was however struck against the wall near which I stood; my body was covered with fire, and I thought for some time I should have been suffocated with smoke, and the smell of sulphur.

*XXX. On a singular Sparry Incrustation found in Somersetshire. By Edw. King, Esq., F. R. S. p. 241.*

In the parish of High Littleton, Somerset, midway between Bristol and Wells, are several coal-mines; and about the end of 1766, a new shaft, or pit, was opened, for conveying air into an adjoining work; but when this shaft was finished, the water that flowed in from the sides, and which at first was taken up by buckets, greatly incommoded the under-works; and therefore the miners at about the depth of 10 fathoms, and just below the place where the water broke in, affixed to the 4 sides of the pit some wooden shoots, about 4 or 5 inches wide, and as many deep; all of them a little inclined towards one corner, where was a hollow perpendicular pipe or trunk of elm, nearly a long square, being about  $7\frac{1}{4}$  inches one way, and  $4\frac{1}{4}$  inches the other; and through this the water, that fell into the lateral shoots, was conveyed down to the level, or passage out; which being about 7 fathoms lower than the shoots, the hollow perpendicular trunk was about 14 yards in length. This trunk having been thus fixed up, in the latter end of 1766, was in about 3 years, or rather less, found to be much stopped up; so that, in August 1769, the miners were obliged to take it up; and then, on taking it to pieces, they found the whole cavity, from one end to the other, nearly filled with a sort of sparry incrustation, somewhat



softer than marble, but harder than alabaster, and which therefore Mr. K. calls a species of marble.

The water, that flowed into the pit on all sides, issued from a stratum of hard brown and reddish sand-stone, replete with shining sparry micæ, and some ocherous matter; and had, in its passage through the trunk, regularly filled up the cavity by slow degrees, with solid incrustations; so that the increase of the marble is marked much in the same manner as the increase of the growth of a tree appears to be, when its trunk is cut horizontally: and at last the water had left only a cavity, which appears in the middle of the block, and which was uniform in its figure from one end of the pipe to the other, and nearly similar to the original cavity; but which, at last, not being large enough to let all the water pass, occasioned the discovery. Since that time, in order to prevent the inconvenience, if possible, a new trunk has been made, larger than the first; and yet, in June 1771, this new trunk also was so far filled up with the sparry incrustation, that there was but just room to thrust 4 fingers into the central cavity; and the lateral shoots, or troughs, also have filled so fast, that they have been obliged every now and then to be cleaned out.

Mr. K. adds the following observations. 1st. As the water flowed in from the shoots, on 2 sides of the square trunk or pipe, it is manifest that the streams must have stricken against each other, at the corner of the pipe where they first met, and also at the opposite corner. And, as it is a known principle of mechanics, that a body, which is acted on by 2 forces, moving in different directions, will describe the diagonal of a parallelogram, of which the directions of those forces is the sides; so here, the line in which the two streams met, and impeded each other's motions, has plainly, as the marble increased, gone on in the diagonal of such a parallelogram from both the corners, viz. from that where the pipe joined the shoots, or troughs, and from the opposite one; but it is also very remarkable, that there is such a diagonal line, not only at these corners, but in like manner at the other two; which can be accounted for no otherwise, than by supposing that each of the 2 streams, dashing against the opposite side of the pipe, formed continually, the whole way down, another stream, in a contrary direction; and so, both together, produced the same effect throughout the whole pipe, as if there had been 4 streams flowing over the 4 sides. On examining the block however, very strictly, it appears, that the lines in the diagonal one way, are stronger than those in the diagonal the other way; and indeed the specimen of the pipe, presented to the Society, has even broken in halves, exactly in one of the diagonals, though the block here described remains entire, and has the appearance of having had its sides joined accurately, in the manner in which a skilful workman would fit 4 boards to be glued together.

2dly. At one place there seems to have been, by some accident or other, the

point of a small nail projecting into the pipe; and here, it is very remarkable, that, either by the dashing of the water, or rather perhaps by an effect which iron has been observed to have of hastening and increasing petrification, the incrustation has gone on faster than in other parts of the same side; but so regularly, that, from the point of the nail to the inner cavity, there is a swelling, or protuberance, so uniform, that it makes throughout nearly the same segment of different circles, of which the point of the nail is the common centre; and that not merely directly opposite to the nail, but throughout this whole block, and even farther downwards.

3dly. The regular increase of these segments of circles is visible in each lamina of the block, and in each lamina the diameter of the circle increases in due proportion; so that it is still nearly the same segment; though, if there be any difference, it is rather a smaller portion of a larger circle; as, from the cause which occasioned it one would be led to expect. And with regard to these laminae it is worth observing, that as they mark the increase of the marble uniformly all round, as the growth of a tree is marked, only the marble increased inward, whereas a tree grows outward, so they seem to have become visible, and to have been thus distinctly marked, by means of the water bringing, at different times, more or less ochre along with the sparry matter: and this is the more probable, as the whole country all round abounds with beds of ochre, and the waters are sometimes much tinged with it.

4thly. The cavity left in the middle of the block is not perfectly similar to the original cavity of the trunk or pipe; because the water did not flow quite uniformly over the edges, at the ends of the shoots or troughs, in consequence probably of their not lying exactly horizontally; whence more water fell upon and against one part of the sides of the trunk, than against the other.

5thly. The outside of the block has taken off impressions of all the roughnesses, knots, and shivers of the elm boards, which composed the trunk or pipe, even more accurately than they could have been taken off by wax, plaster of Paris, or almost any composition whatever, and certainly much more durably. There is in the *Philos. Trans.*, vol. 60, p. 47, (*Abridg.*, p. 10, of this vol.) a very curious paper, from R. S. Raspe, concerning the production of white marble in a similar manner; in which he mentions the taking off impressions of medallions, by means of petrifying waters. And a paper was read at the R. S. some time ago, containing an account of several impressions, actually so taken off in a short time, in durable marble, by means of a petrifying water, near Bologna in Italy: when some of the impressions were also sent, both to the R. S. and to the British Museum. And, as this block here described, and the whole contents of the pipe, of above 40 feet in length, were formed in less than 3 years, there is reason to conclude, that the water of this mine in Somersetshire is as capable

of being improved to the purposes of a new manufactory, as either that near Bologna, or those of Germany and Bohemia. And it is perhaps worth mentioning, that something of this sort has actually been attempted, with good success, in Peru; for we are told by P. Feuillée (who made several curious observations in South America, both phisiological and astronomical, in 1709), that he saw many statues and beautiful vases, or holy water pots, in the churches at Lima, which were simply cast in moulds, by means alone of a petrifying water near Guankabalika, or Guankavelika. And this circumstance is also mentioned in a Description of Peru, published in 1748, a great part of which is taken from Feuillée's account.

6thly. This block of marble takes a very fine polish, as appears by the specimen, the sections of which are polished: and if casts of medals, or other things, were taken in smooth moulds, well formed, their surfaces would therefore probably appear well polished, as those of the medals did, which came from Bologna.

7thly and lastly. Dr. Pococke, in his Travels, describing a very curious grotto in the island of Candia, or Crete, which exceeded all others that he ever saw in beauty, and the slenderness of the pillars, one of which is near 20 feet high, and even transparent, says, "As I had seen stones of this kind hewn out of a rock at Mount Lebanon, which were used as white marble, and appeared to be alabaster, this made me imagine, that when these sorts of petrifications are hard enough to receive a polish, they then become the oriental transparent alabaster which is so much valued, and of which there are 2 curious columns at the high altar of St. Mark in Venice."

*XXXI. Experiments and Observations on the Singing of Birds. By the Hon. Daines Barrington, V. P. R. S. p. 249.*

To chirp, is the first sound which a young bird utters, as a cry for food, and is different in all nestlings, if accurately attended to; so that the hearer may distinguish of what species the birds are, though the nest may hang out of his sight and reach. This cry is very weak and querulous; it is dropped entirely as the bird grows stronger, nor is afterwards intermixed with its song, the chirp of a nightingale, for example, being hoarse and disagreeable.

The call of a bird, is that sound which it is able to make, when about a month old; it is, in most instances, a repetition of one and the same note, is retained by the bird as long as it lives, and is common, generally, to both the cock and hen. The next stage in the notes of a bird is termed, by the bird-catchers, recording, which word is probably derived from a musical instrument, formerly used in England, called a recorder. This attempt in the nestling to sing, may be compared to the imperfect endeavour in a child to babble. This

first essay does not seem to have the least rudiments of the future song; but as the bird grows older and stronger, one may begin to perceive what the nestling is aiming at. While the scholar is thus endeavouring to form his song, when he is once sure of a passage, he commonly raises his tone, which he drops again when he is not equal to what he is attempting; just as a singer raises his voice, when he not only recollects certain parts of a tune with precision, but knows that he can execute them. What the nestling is not thus thoroughly master of, he hurries over, lowering his tone, as if he did not wish to be heard, and could not yet satisfy himself. A young bird commonly continues to record for 10 or 11 months, when he is able to execute every part of his song, which afterwards continues fixed, and is scarcely ever altered. When the bird is thus become perfect in his lesson, he is said to sing his song round, or in all its varieties of passages, which he connects together, and executes without a pause.

Notes in birds are no more innate, than language is in man, and depend entirely on the master under which they are bred, as far as their organs will enable them to imitate the sounds which they have frequent opportunities of hearing. Mr. B. educated nestling linnets under the 3 best singing larks, the skylark, woodlark, and titlark, every one of which, instead of the linnet's song, adhered entirely to that of their respective instructors. When the note of the titlark-linnet was thoroughly fixed, he hung the bird in a room with 2 common linnets, for a quarter of a year, which were full in song; the titlark-linnet, however, did not borrow any passages from the linnet's song, but adhered stedfastly to that of the titlark. Having some curiosity to find out whether a European nestling would equally learn the note of an African bird, he educated a young linnet under a vengolina, which imitated its African master so exactly, without any mixture of the linnet song, that it was impossible to distinguish the one from the other. This vengolina linnet was absolutely perfect, without ever uttering a single note by which it could have been known to be a linnet. In some of his other experiments, however, the nestling linnet retained the call of its own species, or what the bird-catchers term the linnet's chuckle, from some resemblance to that word when pronounced.

Having before stated, that all his nestling linnets were 3 weeks old when taken from the nest; and by that time they frequently learn their own call from the parent birds, which consists of only a single note. To be certain therefore, that a nestling will not have even the call of its species, it should be taken from the nest when only a day or two old; because, though nestlings cannot see till the 7th day, yet they can hear from the instant they are hatched, and probably, from that circumstance, attend to sounds more than they do afterwards, especially as the call of the parents announces the arrival of their food. Mr. B. owns that he is not equal himself, nor can he procure any person to take the trouble of

breeding up a bird of this age, as the odds against its being reared are almost infinite. The warmth indeed of incubation may be, in some measure, supplied by cotton and fires; but these delicate animals require, in this state, being fed almost perpetually, while the nourishment they receive should not only be prepared with great attention, but given in very small portions at a time. Yet he has happened to see both a linnet and a goldfinch which were taken from their nests when only 2 or 3 days old. The first of these belonged to Mr. Matthews, an apothecary at Kensington, which, from a want of other sounds to imitate, almost articulated the words pretty boy, as well as some other short sentences: and Mr. Matthews assured him that he had neither the note nor call of any bird whatsoever.

The goldfinch was reared in the town of Knighton in Radnorshire, which Mr. B. happened to hear as he was walking by the house where it was kept. He thought a wren was singing, and he went into the house to inquire after it, as that little bird seldom lives long in a cage. The people of the house however told him, that they had no bird but a goldfinch, which they conceived to sing its own natural note, as they called it; on which he staid a considerable time in the room, while its notes were merely those of a wren, without the least mixture of goldfinch. On further inquiries, he found that the bird had been taken from the nest when only 2 or 3 days old; that it was hung in a window which was opposite to a small garden, whence the nestling had undoubtedly acquired the notes of the wren, without having had any opportunity of learning even the call of the goldfinch.

These facts seem to prove very decisively, that birds have not any innate ideas of the notes which are supposed to be peculiar to each species. But it will possibly be asked, why in a wild state they adhere so steadily to the same song, in so much that it is well known, before the bird is heard, what notes you are to expect from him. This however arises entirely from the nestlings attending only to the instruction of the parent bird, while it disregards the notes of all others, which may perhaps be singing round him. But, to prove this decisively, Mr. B. took a common sparrow from the nest when it was fledged, and educated him under a linnet: the bird however by accident heard a goldfinch also, and his song was therefore a mixture of the linnet and goldfinch. Mr. B. educated a young robin under a very fine nightingale; which however began already to be out of song, and was perfectly mute in less than a fortnight. This robin afterwards sung 3 parts in 4 nightingale, and the rest of his song was what the bird-catchers call rubbish, or no particular note whatever. He educated a nestling robin under a woodlark-linnet, which was full in song, and hung very near to him for a month together: after which, the robin was removed to another house, where he could only hear a skylark-linnet. The consequence was, that the nestling

did not sing a note of woodlark, though he afterwards hung him again just above the woodlark linnet, but adhered entirely to the song of the skylark linnet. Birds in a wild state do not commonly sing above 10 weeks in the year; which is then also confined to the cocks of a few species: Mr. B. conceives that this last circumstance arises from the superior strength of the muscles of the larynx.

Strength however in these muscles, seems not to be the only requisite; the birds must have also great plenty of food, which seems to be proved sufficiently by birds in a cage singing the greatest part of the year, when the wild ones do not continue in song above 10 weeks. Mr. B. knows well, that the singing of the cock bird in the spring is attributed by many to the motive only of pleasing its mate during incubation. Those however who suppose this, should recollect, that much the greater part of birds do not sing at all: why should their mate therefore be deprived of this solace and amusement? The bird in a cage, which perhaps sings 9 or 10 months in a year, cannot do so from this inducement; and, on the contrary, it arises chiefly from contending with another bird, or indeed against almost any sort of continued noise. Superiority in song gives to birds a most amazing ascendancy over each other; as is well known to the bird catchers by the fascinating power of their call birds, which they contrive should moult prematurely for this purpose.

But, to show decisively that the singing of a bird in the spring does not arise from any attention to its mate, a very experienced catcher of nightingales informed him, that some of these birds have jerked the instant they were caught. He has also brought to him a nightingale which had been but a few hours in a cage, and which burst forth in a roar of song. Yet this bird is so sulky on its first confinement, that he must be crammed for 7 or 8 days, as he will otherwise not feed himself: it is also necessary to tie his wings, to prevent his killing himself against the top or sides of the cage.

Mr. B. believes there is no instance of any bird's singing which exceeds our blackbird in size; and possibly this may arise from the difficulty of its concealing itself, if it called the attention of its enemies, not only by bulk, but by the proportionable loudness of its notes. He rather conceives it is for the same reason that no hen bird sings, because this talent would be still more dangerous during incubation; which may possibly also account for the inferiority in point of plumage.

Mr. B. considers how far the singing of birds resembles our known musical intervals, which are never marked more minutely than to half notes; because, though we can form every gradation from half note to half note, by drawing the finger gently over the string of a violin, or covering by degrees the hole of a flute; yet we cannot produce such a minute interval at command, when a quarter note for example is required. Some passages of the song in a

few kinds of birds correspond with the intervals of our musical scale, of which the cuckoo is a striking and known instance: much the greater part however of such song, is not capable of musical notations. As a bird's pitch is higher than that of any instrument, we are at a loss when we attempt to mark their notes in musical characters, which we can so readily apply to such as we can distinguish with precision. An unsurmountable difficulty is, that the intervals used by birds are commonly so minute, that we cannot judge at all of them, from the more gross intervals into which we divide our musical octave. Though we cannot attain the more delicate and imperceptible intervals in the song of birds, yet many of them are capable of whistling tunes with our more gross intervals, as is well known by the common instances of piping bullfinches, and canary birds.

This however arises from mere imitation of what they hear when taken early from the nest; for if the instrument from which they learn is out of tune, they as readily pipe the false, as the true notes of the composition.

The next point of comparison to be made between our music and that of birds is, whether they always sing in the same pitch. The first requisite to make such sounds agreeable to the ear is, that all the birds should sing in the same key, which he believes they do. Now, of all the musical tones which can be distinguished in birds, those of the cuckoo have been most attended to, which form a flat 3d, not only by the observations of a harpsichord tuner, but likewise by those of Kircher, in his *Musurgia*. Another proof of our musical intervals being originally borrowed from the song of birds, arises from most compositions being in a flat third, where music is simple, and consists merely of melody. The oldest tune Mr. B. heard, is a Welsh one, called *Morvar Rhyddland*, which is composed in a flat 3d; and if the music of the Turks and Chinese be examined in *Du Halde* and *Dr. Shaw*, half of the airs are also in a flat 3d. The music of 2 centuries ago is likewise often in a flat 3d, though 99 compositions out of 100 are now in the sharp 3d. The reason however of this alteration seems to be very clear: the flat 3d is plaintive, and consequently adapted to simple movements, such as may be expected in countries where music has not been long cultivated. There is on the other hand a most striking brilliancy in the sharp 3d, which is therefore proper for the amazing improvements in execution, which both singers and players have arrived at within the last fifty years. When *Corelli's* music was first published, our ablest violinists conceived that it was too difficult to be performed; it is now however the first composition attempted by a scholar. Every year also now produces greater and greater prodigies on other instruments, in point of execution.

Mr. B. before observed, that by attending to a nightingale, as well as a robin which was educated under him, he always found that the notes, reducible to our intervals of the octave, were precisely the same; which is another proof that

birds sing always in the same key. In this circumstance, they differ much from the human singer; because those who are not able to sing at sight, often begin a song either above or below the compass of their voice, which they are not therefore able to go through with. As birds however form the same passages with the same notes, at all times, this mistake of the pitch can never happen in them. Few singers again can continue their own part, while the same passages are sung by another in a different key; or if the same or other passages are sung, so as not to coincide with the musical bar, or time of the first singer. As birds however adhere so stedfastly to the same precise notes in the same passages, though they never trouble themselves about what is called time in music; it follows that a composition may be formed for 2 piping bulfinches, in 2 parts, so as to constitute true harmony, though either of the birds may happen to begin, or stop, when they please.

Mr. B. had observed, that perhaps no bird may be said to sing which is larger than a blackbird, though many of them are taught to speak: the smaller birds however have this power of imitation; though perhaps the larger ones have not organs which may enable them, on the other hand, to sing. And he mentions several expressions among the ancients noticing the speaking of birds.

As it appears from these citations, that so many different sorts of birds have learned to speak, and as Mr. B. has showed that a sparrow may be taught to sing the linnet's note, he scarcely knows what species to fix on, that may be considered as incapable of such imitations; for it is clear, from several experiments before stated, that the utmost endeavours will not be wanting in the bird, if he is endowed with the proper organs. It can therefore only be settled by educating a bird, under proper circumstances, whether he is thus qualified or not; for if one was only to determine this point by conjecture, one should suppose that a sparrow would not imitate the song of the linnet, nor that a nightingale or partridge could be taught to speak.

Considering the size of many singing birds, it is rather amazing at what a distance their notes may be heard. Thus, a nightingale may be very clearly distinguished at more than half a mile, if the evening is calm. Mr. B. has also observed the breath of a robin, which exerted itself, so condensed in a frosty morning, as to be very visible. To make the comparison however with accuracy, between the loudness of a bird's and the human voice, a person should be sent to the spot from which the bird is heard; Mr. B. conceives that, on such trial, the nightingale would be distinguished farther than the man. It must have struck every one, that, in passing under a house where the windows are shut, the singing of a bird is easily heard, when at the same time a conversation cannot be so, though an animated one. Most people, who have not attended to the notes



of birds, suppose that those of every species sing exactly the same notes and passages; which is by no means true, though it is admitted that there is a general resemblance. Thus the London bird catchers prefer the song of the Kentish goldfinches, but Essex chaffinches; and when they sell the bird to those who can thus distinguish, inform the buyer that it has such a note, which is very well understood between them. Some of the nightingale fanciers also prefer a Surry bird to those of Middlesex. These differences in the song of birds of the same species cannot perhaps be compared to any thing more apposite, than the varieties of the provincial dialects. The nightingale seems to have been fixed on, almost universally, as the most capital of singing birds, which superiority it certainly may boldly challenge: one reason however of this bird's being more attended to than others is, that it sings in the night.

In the first place, its tone is infinitely more mellow than that of any other bird, though, at the same time, by a proper exertion of its musical powers, it can be excessively brilliant. When this bird sang its song round, in its whole compass, Mr. B. has observed 16 different beginnings and closes, at the same time that the intermediate notes were commonly varied in their succession with such judgment, as to produce a most pleasing variety. The bird which approaches nearest to the excellence of the nightingale, in this respect, is the skylark; but then the tone is infinitely inferior in point of mellowness: most other singing birds have not above 4 or 5 changes. The next point of superiority in a nightingale is its continuance of song, without a pause, which Mr. B. has observed sometimes not to be less than 20 seconds. Whenever respiration however became necessary, it was taken with as much judgment as by an opera singer. The skylark again, in this particular, is only second to the nightingale. Mr. B. here inserts a table, by which the comparative merit of the British singing birds may be examined, in which the number 20 denotes the point of absolute perfection.

	Mellowness of tone.	Sprightly Notes.	Plaintiff Notes.	Compass.	Execution.
Nightingale .....	19	14	19	19	19
Skylark .....	4	19	4	18	18
Woodlark .....	18	4	17	12	8
Titlark .....	12	12	12	12	12
Linnet .....	12	16	12	16	18
Goldfinch .....	4	19	4	12	12
Chaffinch .....	4	12	4	8	8
Greenfinch .....	4	4	4	4	6
Hedge-sparrow .....	6	0	6	4	4
Aberdavine (or siskin) .....	2	4	0	4	4
Redpoll .....	0	4	0	4	4
Thrush .....	4	4	4	4	4
Blackbird .....	4	4	0	2	2
Robin .....	6	10	12	12	12
Wren .....	0	12	0	4	4
Reed-sparrow .....	0	4	0	2	2
Blackcap, or the Norfolk mock nightingale .	14	12	12	14	14

And here he again repeats, that what he describes is from a caged nightingale, because those which we hear in the spring are so rank, that they seldom sing any thing but short and loud jerks, which consequently cannot be compared to the notes of a caged bird, as the instrument is overstrained. But it is not only in tone and variety that the nightingale excels; the bird also sings with superior judgment and taste. He has commonly observed, that his nightingale began softly like the ancient orators; reserving its breath to swell certain notes, which by this means had a most astonishing effect, and which eludes all verbal description.

It may not be improper here to consider, whether the nightingale may not have a very formidable competitor in the American mocking bird; though almost all travellers agree, that the concert in the European woods is superior to that of the other parts of the globe. As birds are now annually imported in great numbers from Asia, Africa, and America, Mr. B. has often attended to their notes, both singly and in concert, which are certainly not to be compared to those of Europe. It must be admitted, that foreign birds, when brought to Europe, are often heard to a great disadvantage; as many of them, from their great tameness, have certainly been brought up by hand. The soft billed birds also cannot be well brought over, as the succedaneum for insects, their common food, is fresh meat, and particularly the hearts of animals.

Mr. B. has heard the American mocking bird in great perfection at Mess. Vogle's and Scott's, in Love-lane, Eastcheap. This bird had been in England 6 years. During the space of a minute, he imitated the woodlark, chaffinch, blackbird, thrush, and sparrow. He would also bark like a dog; so that the bird seems to have no choice in his imitations, though his pipe comes nearest to our nightingale of any bird yet met with. With regard to the original notes however of this bird, we are still at a loss; as this can only be known by those who are accurately acquainted with the song of the other American birds. Kalm indeed informs us, that the natural song is excellent; but this traveller seems not to have been long enough in America, to have distinguished what were the genuine notes: with us, mimics do not often succeed but in imitations. Mr. B. has little doubt however, but that this bird would be fully equal to the song of the nightingale in its whole compass; but then, from the attention which the mocker pays to any other sort of disagreeable noises, these capital notes would always be debased by a bad mixture.

We have one mocking bird in England, which is the skylark; as, contrary to a general observation before made, this bird will catch the note of any other which hangs near it; even after the skylark note is fixed. For this reason, the bird fanciers often place the skylark next one which has not been long caught, in order, as they term it, to keep the cage skylark honest. The question,

indeed may be asked, why the wild skylark, with these powers of imitation, ever adheres to the parental note; but it must be recollected, that a bird when at liberty is for ever shifting its place, and consequently does not hear the same notes eternally repeated, as when it hangs in a cage near another. In a wild state therefore the skylark adheres to the parental notes; as the parent cock attends the young ones, and is heard by them for a considerable time.

It may be asked, how birds originally came by the notes which are peculiar to each species. The answer however to this is, that the origin of the notes of birds, together with its gradual progress, is as difficult to be traced as that of the different languages in nations. The loss of the parent cock at the critical time for instruction has doubtless produced those varieties, which are in the song of each species; because then the nestling has either attended to the song of some other birds; or perhaps invented some new notes of its own, which are afterwards perpetuated from generation to generation, till similar accidents produce other alterations. The organs of some birds also are probably so defective, that they cannot imitate properly the parental note, as some men can never articulate as they should do. Such defects in the parent bird must again occasion varieties, because these defects will be continued to their descendants, who will only attend to the parental song. Some of these descendants also may have imperfect organs; which will again multiply varieties in the song. The truth is, that scarcely any two birds of the same species have exactly the same notes, if they are accurately attended to, though there is a general resemblance. Thus most people see no difference between one sheep and another, when a large flock is before them. The shepherd however knows each of them, and can swear to them, if they are lost; as can the Lincolnshire gosherd to each goose.

But we may not only improve the notes of birds by a happy mixture, or introduce those which were never before heard in Great-Britain; as we may also improve the instrument with which the passages are executed. If, for example, any bird fancier is particularly fond of what is called the song of the canary bird, which however must be admitted to be inferior in tone to the linnet, it would answer well to any such person, if a nestling linnet was brought up under a canary bird, because the notes would be the same, but the instrument which executes them would be improved. We learn also, from these experiments, that nothing is to be expected from a nestling brought up by hand, if he does not receive the proper instruction from the parent cock: much trouble and some cost is therefore thrown away by many persons in endeavouring to rear nestling nightingales, which, when they are brought up and fed at a very considerable expence, have no song worth attending to. If a woodlark, or skylark, was educated however under a nightingale, it follows that this charge, which amounts to a shilling per week, might be in a great measure saved, as well as the trouble of chopping fresh meat every day. A nightingale, again, when

kept in a cage, does not live often more than a year or two: nor does he sing more than 3 or 4 months; whereas the scholar pitched on may not only be more vivacious, but will continue in song 9 months out of 12.\*

XXXII. *On the Tokay and other Wines of Hungary.* By Sylvester Douglas,† Esq. p. 292.

The town, or rather village, of Tokay, whence this celebrated wine derives its name, stands at the foot, and to the east of a high hill, close by the conflux of the river Bodrog, with the Theis or Tibiscus. In the Norimberg map of Hungary, it is erroneously placed between these rivers, for it is on the west side of both. The inhabitants are chiefly either Hungarians of the protestant religion, or Greeks, who came originally from Turkey, but have been long settled here for the purpose of carrying on the wine trade. The hills on which the wine grows, lie all to the west of the river Bodrog, and beginning close by the town of Tokay, thence extend westward and northward, occupying a space of perhaps 10 English miles square; but they are interrupted and interspersed with a great many extensive plains, and several villages. Near some of these the wine is better than what grows on the hill of Tokay, but it all goes under the same general name. The vineyards extend beyond the 48th degree of northern latitude. The soil, on all the hills where the wine grows, is a yellow clayish earth, extremely deep, and there are interspersed through it large loose stones, which it seems are limestone; but he had not an opportunity of examining them.

As the hills do not run in a regular chain, but are scattered among the intervening plains, all kinds of exposures are met with upon them, and there is wine on them all, except perhaps where they are turned directly towards the south. Yet the general rule is, that the exposures most inclining to the south, the steepest declivities, and the highest part of those declivities, produce the best wine. It is a vulgar error, that the Tokay wine is in so small quantity, as never to be found genuine, unless when given in presents by the court of Vienna. The extent of ground on which it grows is a sufficient proof to the contrary. It is a common dessert wine in all the great families at Vienna, and in Hungary, and is very generally drank in Poland and Russia, being used at table in those countries, like Madeira in this.

Another vulgar error is, that all the Tokay wine is the property of the empress queen. She is not even the most considerable proprietor, nor of the best wine; so that every year she sells off her own, and purchases from the other proprietors, to supply her own table, and the presents she makes of it. The greatest

\* The above is only a short sketch of the principal parts of Mr. Barrington's paper. But the whole of it may be consulted in the 3d vol. of Pennant's British Zoology.

† Now Lord Glenbervie.

proprietor is the Prince Trautzon, an old man, at whose death indeed his estate will escheat to the crown; but many others of the German and Hungarian nobility have large vineyards at Tokay; most of the gentlemen in the neighbourhood have part of their estates there; the Jesuits college at Ungwar has a considerable share of the best wine; and besides these, there are many of the peasants who have vineyards, which they hold of the queen, or other lords, by paying a tithe of the annual produce.

There is never any red wine made at Tokay, and, as far as he recollects, the grapes are all white. The vintage is always as late as possible. It commonly begins at the feast of St. Simon and Jude, October 28, sometimes as late as St. Martin's, November 11. This is determined by the season, for they have the grapes on the vines as long as the weather permits; as the frosts, which from the end of August are very keen during the nights, are thought to be of great service to the wine. By this means it happens, that when the vintage begins, a great many of the grapes are shrivelled, and have in some measure the appearance of dried raisins.

There are 4 sorts of wine made from the same grapes, which they distinguish at Tokay by the names of Essence, Auspruch, Masslasch, and the common wine. The process for making them is as follows. The half-dried and shrivelled grapes, being carefully picked out from the others, are put into a perforated vessel, where they remain as long as any juice runs off by the mere pressure of their own weight. This is put into small casks, and is called the Essence. On the grapes from which the essence has run off, is poured the expressed juice of the others from which they had been picked, and then they tread them with their feet. The liquor obtained in this manner stands to ferment during a day or two, after which it is poured into small casks, which are kept in the air for about a month, and afterwards put into the cellars. This is the Auspruch.

The same process is again repeated, by the addition of more of the common juice to the grapes which have already undergone the two former pressures, only they are now also wrung with the hands, and this gives the Masslasch. The 4th kind is made by taking all the grapes together at first, and submitting them to the greatest pressure. It is chiefly prepared by the peasants, who have not a sufficient quantity of grapes, and cannot afford the time and apparatus necessary for making the different sorts. It is entirely consumed in the country, and forms the common vin du pays.

The Essence is thick, and never perfectly clear, very sweet and luscious. It is chiefly used to mix with the other kinds, and when joined to the Masslasch, forms a wine equally good with the Auspruch, and often sold for it. The Auspruch is the wine commonly exported, and what is known in foreign countries under the name of Tokay. The following are the best rules for judging of it;

though in this and all similar cases, it requires experience to be able to put such rules in practice. 1. The colour should neither be reddish, which it often is, nor very pale, but a light silver. 2. In trying it, you should not swallow it immediately, but only wet your palate and the tip of the tongue. If it discover any acrimony to the tongue, or bite it, it is not good. The taste ought to be soft and mild. 3. It should, when poured out, form globules in the glass, and have an oily appearance. 4. When genuine, the strongest is always of the best quality. 5. When swallowed, it should have an earthy astringent taste in the mouth, which they call the taste of the root. The Poles particularly are fond of this astringency and austerity in their Tokay. There is so great a difference between the Tokay used in Poland and what Mr. D. drank both at Tokay and Vienna, which, he was sure, was of the best and most genuine kind, that he thinks their wine is composed of the Masslasch, which, by the severe pressure it suffers, must carry with it much of the astringent quality which, in all grapes, resides in the skin, and a smaller proportion than usual of the essence. But this is mere conjecture.

Besides the qualities already mentioned, all Tokay wine has an aromatic taste; so peculiar, that nobody who has ever drank it genuine can confound it with any other species of wine. The only species that bears a resemblance to it grows, in a very small quantity, in the Venetian Friule, and is only to be met with in private families at Venice, where, in the dialect of the place, it is called *vin piccolit*. The Tokay wine, both the *Essence* and *Auspruch*, keeps to any age, and improves by time. Mr. D. has drank of the latter at Vienna, which had been in the same cellar since the year 1686. It is never good till it is about 3 years old. All the sorts are generally kept in small casks, called *antheils*, which legally hold 80 Hungarian *mediæ*, a measure containing about two-thirds of an English quart. When you buy it of the gentlemen who are proprietors, you have commonly more than the legal quantity in the *antheil*; if from the Greek merchants, always less.

The particular year, or vintage, and the age, vary the price of this, as of all other wines. The medium price of the *antheil* of *Essence* is between 60 and 70 ducats. It is sometimes sold on the spot for more than 100. Prince Radzivil paid 300 ducats for 2 *antheils* about 4 years before. When the price is 60 ducats, and the *antheil* large measure, that is, about 90 *mediæ*, it is exactly a ducat the English quart. The price of the *Auspruch* is from 26 to about 30 ducats the *antheil*. This is at the rate of two florins, or near a crown the English quart. The variety in the prices of the *Essence* and *Auspruch*, accounts for the opposite accounts of people, who say sometimes that it costs half a guinea, sometimes 5 shillings, on the spot.

There are people who come every year from Poland, about the time of the

vintage, to choose their own wine on the ground, and see it carefully managed. But it is a false opinion of many, that they contract for the wine of several years forwards: no such thing has ever been practised. For these last 20 years the court of Petersburg has had an agent, who resides constantly at Tokay, for the purpose of buying wine. He commonly purchases every year from 40 to 60 antheils of Auspruch, but never of any other sort.

It is much the best way to transport it in casks; for when it is on the seas, it ferments 3 times every season, and refines itself by these repeated fermentations. When in bottles, there must be an empty space left between the wine and the cork, otherwise it would burst the bottle. They put a little oil on the surface, and tie a piece of bladder on the cork. The bottles are always laid on their sides in sand.

Mr. D. is persuaded an English merchant, or company of merchants, would find their account in establishing a correspondence with one of the principal proprietors in the country, or in sending an agent to reside at Tokay, who might watch the opportunity of the good vintages, choose the best exposures, and bargain with the proprietors themselves. They should have cellars there to keep the wine to a proper age, and an agent at Warsaw, and another at Dantzic, to receive it. This is the road it must take.

There is not, Mr. D. believes, in Europe any country which produces a greater variety of wines than Hungary. They count as many as 100 different sorts. The most valuable white wines, after the Tokay, are, 1. *The St. George wine*, which grows near a village of that name, about 2 German miles north of Presburg, and in the same latitude with Vienna. This wine approaches the nearest of any Hungarian wine to Tokay. Formerly they used to make Auspruch at St. George; but this was prohibited by the court about 16 years ago, it being supposed that it might hurt the traffic of the Tokay wine. 2. *The Edenburg wine*, resembling the St. George, but inferior in quality and value. Edenburg is a town situate about 9 German miles north-west of Presburg. 3. *The Carlowitz wine*, something like that of the Cote rotie on the banks of the Rhone. Carlowitz is the seat of the metropolitan of the Greek church in Hungary. It stands on the banks of the Danube, between 45 and 46 degrees of latitude.

The best red wines are, 1. *The Buda wine*, which grows in the neighbourhood of the ancient capital of the kingdom. This wine is like, and perhaps equal to, Burgundy, and is often sold for it in Germany. A German author of the last century says, that a great quantity of this wine used to be sent to England in the reign of James I., over land by Breslaw and Hamburg, and that it was the favourite wine both at court and all over England. 2. *The Sexard wine*, a strong deep-coloured wine, not unlike the strong wine of Languedoc, which is said to be sold at Bourdeaux for claret. The Sexard wine on the spot costs about

5 creuzers, or  $2\frac{1}{4}$  d. a bottle. It belongs to the Abbot of Constance, and is chiefly consumed in Germany. Sexard is on the Danube, between Buda and Esseh. 3. *The Erlaw wine*, which is reckoned at Vienna almost equal to that of Buda. Erlaw is in Upper Hungary, south-west of Tokay, between 47 and 48 degrees of latitude. 4. *The Gros Wardein wine*, a strong bodied wine, and very cheap. It belongs chiefly to the Duke of Modena, whose ancestor got a large estate in this country, in grant from the Emperor Leopold, as a reward for his services in the Hungarian wars. Gros Wardein is an old fortress near the confines of Transylvania, between 46 and 47 degrees of latitude.

*XXXIII. On the Figure and Composition of the Red Particles of the Blood, commonly called the Red Globules. By Mr. Wm. Hewson, F.R.S. p. 303.*

This paper is reprinted in Mr. Hewson's collected works.

*XXXIV. On the Effects of a Thunder-storm, March 15th, 1773, on the House of Lord Tylney at Naples. In a Letter from the Hon. Sir Wm. Hamilton, F.R.S. Dated Naples, March 20, 1773. p. 324.*

This accident was on his lordship's assembly night; so that most of the nobility of this country, many of the foreign ministers, foreigners of distinction, particularly English, were present at the time of the explosion; there were not less than 250 in the apartments; and including servants, the whole number under Lord Tylney's roof could not be less than 500. The lightning passed through 9 rooms, 7 of which were crowded with parties at cards, or conversing; it was visible in every one, notwithstanding the quantity of candles, and has left in all evident marks of its passage. Many of the company were sensible of a smart stroke, like that of electricity, and some complained for several days after of a pain they felt from that stroke, but no one received any essential hurt; a servant indeed of the French ambassador's house has a black mark on his shoulder and thigh, from a stroke he received on the staircase; and another servant, who was asleep on the same staircase, his head reclining against the wall, had the hair entirely singed from it on that side.

The confusion at the moment was very great: the report, which seems to have been equally heard in every room, was certainly as loud as that of a pistol; and every one flying the room they were in, thinking the danger there, met of course in the door-ways, and stopped all passage. A Polish prince, who was playing at cards, hearing the report, as he thought of a pistol, and feeling himself struck, jumped up, and clapping his hand to his sword, put himself in a posture of defence. Sir Wm. H. was sitting on a card-table, and conversing with M. de Saussure, Professor of Natural History at Geneva; they happened to be looking different ways, and each thought that the bright light and report was immedi-



ately opposite to him; and every one was persuaded that the greatest explosion had been directly before himself. Hearing however a voice saying, un fulmine, un fulmine! they began to examine the gallery in which they were, and soon discovered that the gilding of the cornish had been affected, for in the corners, and at every junction, it was quite blackened; those that had been sitting under the cornishes were covered with the shining particles of the varnish that went over the gilding, and which was thrown off in small dust, at the moment of the explosion. In the apartment above, the same operation had been performed on the gildings; and it is certain that the profusion of gildings, and the bell-wires, prevented the lightning from making more use of the company to conduct it in its course. For further particulars, see Sir Wm. Hamilton's Essays collected.

*XXXV. On some Improvements in the Electrical Machine. By Dr. Nooth. p. 333.*

It is evident, that the electric matter is excited in the instant that the glass passes over the rubber, and that it becomes sensible to us by its adhering to the revolving surface of the glass. It also appeared highly probable, that the quantity of fire, which we find on the glass in motion, is not the whole of that which is excited by the passage of the glass on the rubber. The luminous appearance in the angles between the glass and rubber, and which is extremely distinct in a dark room, rendered it next to certain, that a part of the excited electric fluid returns immediately to the cushion without performing a revolution with the glass; and that of course a circulation of the fire is thus kept up in the substance of the cushion in the common method of constructing the machines.

To be convinced of this, Dr. N. attempted to make the passage of the fire from the glass to the anterior part of the cushion, or to that part which corresponds with the ascending side of the cylinder, demonstrable, by placing a piece of silk between the glass and cushion. This silk was larger than the cushion; and part of it was allowed to adhere, by the attraction of the electric fire, to the ascending part of the cylinder. His view in doing this was to cut off, in that part, the immediate communication between the excited glass and cushion, and by that means render the circulation of electric matter visible, which he suspected to take place in the machine; as it was thus forced to turn over the loose edge of the silk before it could return to the cushion. The event answered his expectation; and he then perceived, that the greatest part of the excited fluid was commonly re-absorbed by the fore part of the cushion without becoming sensible on the superior part of the glass.

Having thus verified his supposition by actual experiments with silken flaps of different sizes, he endeavoured to discover a method of preventing that circulation of the electric fluid, and if possible, of obliging the whole, or the greater part of it, that is once excited, to make the revolution with the glass. This

indeed the silk, when of considerable breadth, in some measure effected; but he thought that this obstruction to the immediate return of the fire might be rendered more complete by increasing the thickness of the silk, or by applying to it some nonconducting substance, that might confine the excited fluid more perfectly to the surface of the revolving cylinder. Bees-wax being a nonconducting substance easily procured, he rubbed the silken flap with it, and found that the return of the fire to the cushion at the anterior part of the machine was by that means much diminished, and consequently the excitation of the glass was apparently increased. The addition however of more silk was still more effectual, in confining the fire to the glass; and when it was employed 10 or 12 times doubled, it seemed to deny any passage from the glass to the cushion.

As Dr. N. thus discovered the method of remedying the common defect in the construction of the anterior part of the cushion, he next attended to that part which corresponds with the descending side of the cylinder. Being convinced that this part of the rubber was alone concerned in the excitation, he imagined that the reverse of what was necessary anteriorly should be adopted in the structure of the posterior part; that instead of placing nonconducting substances between the glass and cushion, we should here make the afflux of the electric matter as great as possible, by the application of the most perfectly conducting bodies. Confining therefore the amalgam to that place where the glass first comes in contact with the rubber, he placed some tinfoil close to the amalgam, and bending it back, secured it to the metallic plate below the cushion. By this means the electric matter found an easy access to the place of excitation; and the effect of the machine was thereby greatly increased. A piece of leather, covered with amalgam, and fixed to the posterior part of the rubber, in such a manner as to allow about an inch of it to pass under the cylinder, answered every purpose of the tinfoil; and, as it was not liable to be corroded by the mercury, like tinfoil, it was on that account much preferable.

From the above experiments it was apparent that the excitation was altogether performed by the posterior portion of the cushion; and that the anterior part, when made of conducting substances, re-absorbs the greater quantity of the excited matter. In the structure therefore of electrical machines, we should always have a free electric communication behind, to facilitate the excitation; and the most perfectly nonconducting substances before, to prevent the re-absorption. To answer these intentions, it will perhaps be advisable to make the cushion of silk, stuffed with hair, and to lay some metallic conductor round the posterior part, that a free access may be allowed to the electric matter coming to the place of excitation from the inferior part of the machine. Cushions, made in this manner, and then covered with silk 10 or 12 times doubled, are much more powerfully excitant than any others that he had yet tried. Various other methods

however may be pursued in the construction of the rubber; but it should be an invariable rule, to place nonconducting bodies before, and conducting substances behind, the cylinder. From the preceding principles, it follows, that the support to the rubber should likewise have its conducting and nonconducting side. For this purpose, it may be necessary to employ baked wood, and to cover the posterior half with tinfoil. The place of excitation will be thus sufficiently supplied with electric matter, and the cylinder will not be robbed of a part of the excited fire, before that fire has made a revolution with the glass.

By attending to the place where the excitation is effected, it must appear evident, that the amalgam is only to be laid on the posterior part of the cushion; its presence indeed would be useless, if not injurious, in any other situation. It will however be found somewhat difficult to confine the pure amalgam to the posterior part of the rubber; but if it is mixed with a little hair powder and pomatum, it pretty well keeps its place. The strewing the amalgam thus prepared on the glass, as it revolves, is perhaps the best method of applying it; as, by that means, it is in a great measure prevented from passing on to the nonconducting substances that are placed before. Should any of the amalgam be carried forward by the revolution of the glass, it should be carefully removed. The necessity of keeping that part free from conducting bodies cannot be too much insisted on; and when fresh amalgam is applied as before mentioned, to the proper part of the rubber, the flap should be held down during half a dozen turns of the machine, lest it might collect some of the amalgam before it is properly fixed. It is a probable conjecture that, when the flap of silk is covered with amalgam, part of the amalgam, which is not immediately subservient to the excitation, acts as a conductor in restoring the fire again to the cushion; and that thus, by an improper disposition of it, we suppress, instead of increasing, the quantity of the excited matter.

In short, when an electrician attends to the preceding principles in the construction of his rubber, and to the proper disposition of the amalgam, he has nothing to fear from the humidity of the atmosphere, as his machine will work equally well in all kinds of weather. The rest of the electrical apparatus may be made according to the directions that have been given by the different electrical writers. Each has had his favourite machine; and perhaps no one has been yet contrived that has not had its peculiar advantages.

*XXXVI. Properties of the Conic Sections; deduced by a Compendious Method. Being a Work of the late Wm. Jones, Esq., F.R.S., which he formerly communicated to Mr. J. Robertson, Libr. R. S., and by him addressed to the Rev. N. Maskelyne, F.R.S., &c. p. 340.*

It is well known that the curves formed by the sections of a cone, and there-

fore called conic sections, have, from the earliest ages of geometry, engaged the attention of mathematicians, on account of their extensive utility in the solution of many problems, which were incapable of being constructed by any possible combination of right lines and circles, the magnitudes used in plane geometry. The properties of these curves are become far more interesting within the last 2 centuries, since they have been found to be similar to those described by the motions of the celestial bodies in the solar system.

Two different methods have been taken by the writers who have treated of their properties; the one, and the more ancient, is to deduce them from the properties of the cone itself; the other is to consider the curves, as generated by the constant motion of 2 or more straight lines moving in a given plane, by certain laws. There are various methods of generating these curve lines in plano; one method will give some properties very easily; but others, with much trouble: while, by another mode of description, some properties may be readily derived, which, by the former, were not so easily come at: so that it appears there may be a manner of describing the curves similar to the conic sections, by the motion of lines on a plane, which in general shall produce the most essential properties, with the greatest facility.

That excellent mathematician, the late Wm. Jones, Esq. F.R.S. had drawn up some papers on the description of these curves, or lines of the second kind, very different from what he gave in his *Synopsis Palmariorum Matheseos*, published in the year 1706; or from that of any other writer on this subject. A copy of these papers he let Mr. R. take about the year 1740, who, though they were in an unfinished state, thought them of too much consequence to be lost; and therefore was desirous of preserving them in the *Phil. Trans.* in the manner he at first transcribed; though he is aware they might have been put into a form more pleasing to the generality of readers: Mr. R. indeed annexed larger diagrams than what accompanied the author's copy, in order to render the lines more distinct, as all the relations are to be represented in a single figure, of each kind. Mr. R. then proceeds to state that Mr. Jones, having laid down a very simple method of describing these curves, seems to have been desirous of arriving at their properties in as expeditious a way as he could contrive; and therefore he has used the algebraic method, in general, of reducing his equations; and on some occasions has used the method of fluxions, to deduce some properties chiefly relating to the tangents; and by a judicious use of these, he has very much abridged the steps which otherwise he must have taken, to have deduced the very great variety of relations he has obtained: these he intended to have arranged in tables, whence an equation expressing the relation between any 3 or more lines of the conic sections, might be taken out as readily as a logarithm out of their tables; this he has only partly executed; but it may easily be con-

tinued by those who are desirous to have it done, and are sufficiently acquainted with what follows. Mr. Jones first gives the organical description of the lines of the 2d kind, or curves of the 1st kind, in the following manner.

*The Description of Lines of the Second Kind.*

Let the right lines  $AD$ ,  $AQ$ , be drawn on a plane, at any inclination to each other. See pl. 7, fig. 13, 14, 15. In  $AD$ ,  $AQ$ , take  $Aa$ ,  $AM$ , of any given magnitude, and draw  $MN$  parallel to  $AD$ . On the points  $A$ ,  $a$ , let two rulers  $AP$ ,  $aP$ , revolve, and cut  $MN$ ,  $AQ$ , in  $N$  and  $Q$ , so that  $AQ$  be every where equal to  $MN$ . Then shall the intersection  $P$  of the rulers describe lines of the 2d kind, or curves of the first kind.

Where the right line  $Aa$ , is the first, or transverse diameter. The point  $c$ , bisecting the diameter  $Aa$ , is the centre. The right line  $PD$ , drawn parallel to  $AQ$ , is the ordinate to the diameter  $Aa$ . The part  $AD$ , or  $CD$ , of the diameter, is the absciss, when reckoned to begin from  $A$  to  $c$ , or from  $c$  to  $A$ . The right line  $nb$  drawn from the centre  $c$  parallel to the ordinate  $PD$ , and terminated in the curve, is called the 2d, or conjugate diameter. Those diameters to which the ordinates are perpendicular, are called the axes. And  $AM$  is the parameter to the diameter  $Aa$ .

From this description, Mr. Jones then proceeds to deduce the several numerous properties of these lines, in short algebraical expressions or equations: but these, in the present advanced state of the conic sections, may well be spared on this occasion.

*XXXVII. An Essay, towards Elucidating the History of the Sea Anemonies.\**

*By Abbé Dicquemare, Prof. of Exper. Philos., &c. at Havre de Grace. Translated from the French. p. 361.*

There is great confusion in the descriptions which naturalists have given of these animals, and no less in the names bestowed upon, and the divisions or classes assigned to them. Some have called them sea-nettle, *urticæ marinæ*, though these animals are not prickly, as some of the wandering nettles are. Other writers have called them sea anemonies. The sea anemonies found on the coast of the Havre seem to constitute 3 different species. Those here put in the first class, because in certain positions they resemble most the flower known by the name of anemone, cling or adhere to rocks and stones, and are often found in the holes that chance to be in them, and seem to like the surface of the water. The outer shape of the body of this animal, when it contracts itself, is much like a truncated cone, pl. 8, fig. 1,† with its basis fixed and strongly clinging to the rock. Its upper part is terminated with a hollow. This cone is often perpendicular to its basis; sometimes it lies in an oblique position to it, or the basis spreads itself irregularly; so that from a round, it alters to an elliptical shape. Sometimes it imitates pretty exactly the inclosing out-leaves of anemo-

\* The Sea Anemonies belong to the genus *Actinia*, and are by no means uncommon on most of the European coasts.

† This seems to be the *Actinia Mesembryanthemum* of Ellis.

nies, while the limbs of the animal are not unlike the shag, or inner part of these flowers, fig. 2. At other times it assumes the shape expressed by fig. 3. Indeed these animals alter their forms so often, that it would be difficult, perhaps even impossible, to describe them exactly. One part of their body or limbs swells at times very considerably, at the expence of the rest. The figures and the particular observations will supply what is wanting here. With regard to their colours, they vary amazingly. Every hue of purple, green, brown and violet is to be seen blended together. A great number of them are of one uniform colour; while others are spotted either symmetrically, as in stripes, or in an irregular, but always pleasing manner. Most of them have round their basis a blue or white streak, broader or narrower, which produces a sort of ring. When many of these animals are put together at the bottom of a flattish and wide vessel, the whole appears as a bed of anemonies.

The sea anemonies of the 2d species are pretty nearly shaped out as those of the 1st, but they are much larger. Mr. D. had some, kept in sea water, that were 18 or 20 inches in circumference. Their cloak or outer skin is rough like shagreen, or full of little knobs.\* See fig. 10, 11, 12, where they are shown in half the natural size. They remain in the sand, sticking to the loose stones in it, and stretch out their limbs to the top, in order to lay hold of their prey, as soon as it touches the superficies of the sand. The flower of poppies is said to be the plague and distress of painters, to represent exactly the variety and brilliancy of its colours; the same may be said of the sea anemonies of this larger species. The purest white, carmine, and ultramarine, would hardly be bright enough to paint them properly. The limbs of some of them are of a moderate or dim colour, at the same time that the cloak is made up of the brightest colours.

The 3d species seems to deviate a little more from the 2d, than this from the 1st. Its body, not unlike for shape and colour to the stalk of a mushroom, is terminated in its lower part by a basis, which the animal fixes to the stones in the sand, while by lengthening out its body, it affords means to the superior part, where the limbs and the mouth are placed, of spreading out and opening themselves at the surface of the sand. See fig. 4.† This species has some slight variety in point of shape, and still more of colour. Some have their limbs of a bright white, or fine violet colour; others of an ivory white. Some are found of the same sort of yellow with the inside of melons. Some are greenish, or of a fine brown, with the middle white, which gives them a likeness to auriculas.

Others again have their limbs of a greyish tint, somewhat like the inside of a broken piece of silver; or alternately mixed with black and white in the manner of the quills of a porcupine.

\* This species is the *Actinia crassicornis*.

† There is no fig. 4 in the original plate.

What first offered itself to Mr. D.'s observations, is what distinguishes these animals from plants, viz. progressive motion, by the help of which they can shift their place; the other determinate motions, by which they are enabled to lay hold of their prey; the means they make use of to defend themselves; their deglutition, digestion, evacuations, and lastly the propagation of their species, &c. What little he has had an opportunity to see of those functions, appears sufficient to place these creatures in the class of spontaneous animals, rather than in the dark indeterminate list of zoophytes.

In May 1772, he clipped all the limbs of a purple anemone of the 1st species. Soon after, these limbs began to bud out again. The 30th of July they were clipped a 2d time, and grew again in less than a month. Having cut them a 3d time, they had a 3d shooting out. The same experiment on a green anemone had the like success. It seems these reproductions might extend as far, or be as often repeated, as patience and curiosity would admit. Several experiments have convinced him that one single limb of these anemonies being cut off, retains a power to fasten itself to any small body that is brought near it, either by its end, or by the side towards the end, but not by that part where the clipping was made. This induces him to think that the effect is produced by suction, rather than by any glutinous matter, which might be supposed to ooze out at the pores. This limb, after being cut off, has also a power to stretch or contract itself alternately.

July the 12th he cut one of these purple anemonies through the body, rather nearer the basis. This part remained adhering to the side of the vessel in which it was; and for several days made various motions. At last it got loose, and then fastened in another place. The 27th it began again to move about, till the end of August, when it became as it were lifeless, very flabby, and had often an offensive smell. He concluded it to be dead; but as it did not lose its shape, he resolved to keep it, and to shift it every day into some fresh sea water. From time to time, he thought it had some sort of motion, and in the beginning of November these motions became more perceptible. It shifted its position, when contrary to its natural state. November the 28th, this stump climbed up to the top of the vessel. He then began to perceive some new limbs growing out. January 13, 14, and 15, 1773, it again moved about; and on the 16th, seeing these growing new limbs, he offered them some bits of muscles, which however were neither eaten, nor even laid hold of. That same day, after several motions in various directions, it loosened its adhesion, and remained motionless and flabby, but without any bad smell, till the beginning of February, when it appeared adhering, but weakly, to the bottom of the vase. The 16th, after several motions, it climbed up to the top, where it remained till the 11th of March, and then loosened its hold. These alternate stations and motions lasted till the

8th of April, without showing any plain and full reproduction. However, the animal continues to grow stronger and thicker. He owns it was very wrong to throw away, a few days after the operation, the upper part that had been cut off. But he did not foresee what would happen.

November the 9th, 1772, he clipped a brown anemone through the body. The basis, together with that part of the stump which was left to it, shrunk up, as in fig. 5, and remained motionless where it was at first, till January 13, when it shifted its place. The 15th, he very distinctly perceived 2 rows of limbs growing out of the part where the section was made, fig. 6, and the animal moved along. The next day he offered it bits of muscles, which it laid hold of and ate. These growing limbs were, at first, of a sullied white; but they became browner and browner every day; and are at present of the same colour with the coat of the animal. They are pretty near as large as they were before the operation; but he had not perceived, as yet, some of those small fine blue knobs, that are to be seen round the rim or upper knurl of the coat, as may be seen in fig. 2. As to the part cut off, seen in fig. 7, which consists of about half the body, and wherein the limbs and mouth are placed, he offered it, after the operation was performed, that brown part of a muscle, by the help of which it moves along; and whence the beard spreads out. This bit, which is not easily digested by sea-anemonies, was directly snapped up by the limbs. They drew it to the mouth, which lengthened itself out to catch it, and swallowed it down. But, as the body was wanting to receive it, the bit came out at the opposite end, just as a man's head, being cut off, would let out at the neck the bit taken in at the mouth. He offered it a 2d time, and the animal swallowed it again; but threw it up at the mouth the next day. He still kept that part of the anemone, which daily grew stronger and stronger, and which appeared to suck in the bits of muscles he offered it. The limbs lay hold of them, and the mouth takes them in, either whole or in part, and throws them up a good deal altered; and pretty often these bits go through as they did the first time. Some persons, who were eye-witnesses of these particulars, were of opinion that, from the remains of organization and habit, this part of the animal still endeavoured to gratify a natural want, though no longer subsisting; but Mr. D. is inclined to think that it still exists. In his opinion the part is nourished by means of suckers, of which he suspects it to be full, both inwardly and outwardly. He was in expectation to have his conjecture confirmed by experience, and by it to be enabled to convince other people. The microscope seems to have already corroborated his notion on this subject. When the limbs of these anemonies, especially those of the 2d species, are touched, the person's fingers are felt to adhere and strongly to stick to the limbs. Mr. D. therefore let both these, and several other species of these animals, fasten several times on the fingers of one of his hands, in order to see



whether any glutinous matter should remain on them; but he never perceived any; and by applying the fingers of that hand to the other, he perceived no adhesion. Mr. D. afterwards, for some days, clipped several anemonies of the first species, diametrically and perpendicularly to the basis. They stood the operation extremely well. Time will teach us what the result of these operations will be.

These animals can live a whole year, and perhaps much longer, without any other food than what they chance to find disseminated in the sea-water. They do not want many motions to procure their food, besides stretching out their limbs, to receive such as comes within their reach; and they remain surrounded with muscles, &c. without laying hold of any of them. He has given anemonies some of these muscles alive, but with their shells closed, and about six lines in length. They were swallowed in that state; and 40, 50, and 60 hours after, the shells were thrown up at the mouth, empty and perfectly cleared, even from the small tendons which connect the fish to its shells. The anemonies swallow and digest small fish, and bits of larger fish, or of raw meat, when offered to them. When they cannot digest some of the food, they throw it up at the mouth, either whole or partly dissolved into a viscous liquor, which may in some measure be considered as their excrements. They void by the same way, and in the same manner, various parts of a white and brown substance, and small bodies are thrown out at the extremity of the limbs, and more sensibly in the 2d species. There also oozes out of their body a sort of excrementitious matter, which by coagulating produces round them a sort of girdle.

These animals are known to be viviparous. Several of them have brought forth, even in Mr. D.'s hand, 8, 10, and 12 young ones. Some, though almost imperceptible, as in fig. 8, have even the power of clinging, and are endowed with 2 rows of limbs, which they open immediately on their birth, in order to catch their prey, which they swallow afterwards. He has kept some for 10 months. They have appeared not to have increased in their bulk more than twice the diameter of their size. Indeed he fed them sparingly, the others grew as large as half a green pea. Fig. 9.

The sea anemonies have a progressive motion; which, though slow, is performed in every direction, with a degree of facility, and effected by means of muscles which cross each other at right angles. Mr. D. cannot as yet display the mechanism of these muscles, because the last mutilations he was obliged to make discompose the conjectures that some learned men have published on this subject; and he had not sufficiently fixed his thoughts in consequence of his observations.

The 2d species of sea-anemonies keeps itself hid more than the 1st, it is not to be come at but in neap tides, when the sea recedes farthest, and cannot be

so easily observed. With great difficulty are these anemonies loosened from the stones they adhere to; part of the basis is often left behind, and they are not easily preserved at home. Seeing however some of the individuals that voided muscle shells whole, and still joined together, but empty, he found out the way to feed them. Crab shells, about the size of a hen's egg, are likewise discharged whole; and on offering them some live ones, he found that they swallowed them down, and voided the remains sucked dry in about 20 hours. He cut open some of this 2d species, which he could not loosen from the stones, and among them he found one that had swallowed an anemone of the 3d species; but this had received no harm. For, having put it into some sea-water, it opened and spread as usual. He offered them several of the same species, which they swallowed down; but threw up again alive within 8, 10, or 12 hours, or even later. Is then a live anemone an undigestible body for another?

On the 21st of June, 1772, having caught the instant that an individual of the 3d species was stretching out, as expressed in fig. 13, he snipped off at once with sharp scissors the whole upper part where the limbs and mouth are placed. It was with great satisfaction that, 8 days after, he perceived new limbs growing out, as in fig. 15. The 3d of July, the animal began to eat some bits of muscles; and towards the middle of the month, the upper part was so completely formed, that it might easily have been mistaken for one of its unclipped neighbours, had there been many in the glass. It is neither the row of the central or inner limbs, nor the most outward, which first bud out, but the intermediate ones. The part which had been clipped off gave signs of sensibility to the 17th of July, contracting and dilating itself, in the same manner as a whole anemone; but it was much smaller than before the operation. This experiment has been repeated by clipping, on the 11th of July, the whole upper part and one-third of the body of another anemone. New limbs began to shoot out the 21st. There were 2 rows of them on the 25th; and on the 3d of August, 4 very distinct and well shaped, which caught and kept fast the food that was offered the animal. The mouth itself was sufficiently well formed to take in several times bits of muscles. On the 11th, he perceived in the limbs the faint alternate marks of ivory white and black, and soon after, there scarcely appeared any sign of the operation having been performed.

Being induced to try further experiments, on the 7th of August he clipped an anemone across the body. Like the others, it moved or wriggled a little at first: but he did not perceive any new limbs growing till towards the end of the month. During that time, it continued in such a state as gave but little hopes of its doing well again. Two rows of limbs appeared at last, and the insect recovered its strength. There was on the 9th of September a 3d row of limbs, and the mouth appeared to be shaped out and formed; yet the anemone neither

ate nor kept the bits of muscles he gave it. A 4th row was growing out on the 19th, which gathered strength by degrees; so that on the 3d of October it began to eat, and in a short time became a complete animal. On September the 22d, the upper part appeared to be withering away. But he soon saw how much he had reason to think he was mistaken.

He cut another anemone, of the same species, across the middle of the body, in such a manner as that the 2 parts were only left hanging together by one-fourth part of the diameter. His design was, to try whether nature would produce limbs on the edge of the lower half-part, in the same manner as when the body is cut quite asunder; or whether the wound, though very deep, would heal up again. Nature was not wanting to itself; for, notwithstanding the largeness of the incision, the 2 severed parts were joined up together, and in a few days the wound was healed up. The animal did not even seem to have suffered so much as one might have expected.

Mr. D. witnessed a fact remarkable enough to be inserted here. Having sliced a bit of fish, he offered one end of it to an anemone, whose limbs are of the melon-yellow colour, and the other end to a grey anemone, whose superior part had been reproduced after it had been cut off. The 2 insects, which were adhering pretty near each other at the bottom of the glass, directly seized their prey with their arms; but the yellow one happened to lay hold of the larger share of the slice. Each swallowed on, by the respective ends, till at last each other's mouth came within contact. The grey one seemed at first to get the better; but the other soon recovered her share, then lost it again, and again recovered it. These alternate victories lasted about 3 hours: and there was a time, during which the yellow anemone was nearly worsted; till at last, the grey one losing hold of her end of the slice, the other carried off the prize. Yet, as she sucked in but slowly, the grey one ventured with her mouth on a last tug at her end, still in sight, which she had slipped; but this fresh effort proved fruitless; the yellow champion gave a last pull, and swallowed down the whole. During this whole strife, the two parties did not seem to be animated by any other passion than that of snatching the slice of fish from each other; and though the 2 animals continued afterwards to remain neighbours, they lived very quiet and peaceably together.

These animals are sometimes very voracious. Could it be believed that the same creature that can continue in pretty good plight for a whole year, and perhaps longer, without taking in any other food, besides what may be disseminated in the sea-water, and does not seem very active to lay hold of its prey, but rather waits patiently till chance throws it within reach of its limbs; that this animal should still be so greedy as to gulp down 2 whole muscles, which he gave to one of them by piece-meal, and burst the next day with indigestion, when it

has a power of throwing up so easily what it has swallowed down? This was the case with an anemone of the 3d species, and of a middle size, which had been fished lately.

On the 8th of October, the same sea anemonies (of which he had clipped out the upper part with the mouth and limbs, instead of which new ones were reproduced, perfect enough to enable them to eat) were divided a 2d time, and again renewed, so as not to be distinguished from those which had undergone no operation. And having taken particular care to feed one of these halves clipped the 2d time, he saw it grow stronger and stronger every day, and perform with equal facility the same functions as any other complete original anemone. The only difference or exception is, that its basis is not yet perfect enough, to enable the animal to adhere or fix itself to the glass; he makes no doubt however, but this new anemone will, in a short time, acquire the only powers yet wanting, to render it a perfect one, see fig. 16 and 17. Might not we ask, on all these facts, what is become of the original animal? is it that which continued adhering by its basis to the glass? or is it the upper half? Are there animals among which an individual is not a simple being?

P. s. Just as this essay was concluded, Mr. D. found out a 4th species of sea anemonies, of the size of the 2d, and of an elegant form, having the appearance of a cluster of white or flesh-coloured feathers.\* These anemonies are found in oyster beds, &c. He observed that there grows, or comes out of their body and mouth, a sort of threads about the size of a horse-hair, which being examined with a solar microscope of 5 inches diameter, appear as if made up of a prodigious number of vessels, in which a liquor is seen to circulate. The largest of these unite together, much in the same manner as the optic nerves do in man. Such an organization is doubtless intended for very important purposes. Some young ones of this species, which still adhered to each other by a string of communication, shut themselves up in the same instant, when this string was touched in the middle. As he could not directly contrive a total section of this large species, he tried it on the young ones; and these shoot out again after the operation; and so have the old ones done since. Mr. D. has met with a sort of monster among these anemonies, viz. one which seemed to inclose or contain 3 others, 2 of which were united at their basis, and the 3d lay, as it were, concealed in the folds.

Nature has resources little known to us; it seems some times to vary its operations, with an intent, as it were, the more to stimulate our curiosity, and perhaps to disclose her secrets to those who are endowed with a degree of sagacity or patience, sufficient to follow and investigate the effects offered to our ob-

\* This species is the *Actinia plumosa*.

servations. Among the great number of anemonies of the 3d species, which Mr. D. had clipped across the body, there happened to be 2, whose lower part has in the usual way shot forth new limbs; but the upper half, where the limbs and mouth were, instead of healing up into a new basis, has produced both another mouth and limbs. Hence an animal was formed, which caught its prey, and fed by both ends at the same time.

The sea anemonies of the first 3 species mentioned before, and perhaps those likewise of the 4th, feed on those floating transparent animals of a white glassy, or of a blue or purplish hue, called wandering nettles, or sea jellies. An anemone of a middle size, of the 1st and 3d species, such as that represented by fig. 1 and 13, swallows one of these animals of the size of half an orange. All these 4 species are good to eat.

*Particular Explanation of some of the Figures.*

Fig. 14 shows the anemone of the 3d species, when shrunk up. One sees round it a ring of sand and broken pieces of shells sticking together by means of the excrementitious humour habitually oozing out of the body of the animal, or out of the little granulated knobs, with which it is covered towards the upper part. This ring is also to be seen in the same anemone, when lengthened out, as expressed in fig. 13.

Fig. 10\* shows a sea anemone, of the 2d species, concealed under the sand, and covered over in different places with broken shells and gravel, with which the animal forms a coat of mail to secure itself under, but out of which it can slip in an instant. The figure shows it when it spurts out water at its mouth, and at the end of its limbs.

Fig. 11\* shows the same anemone open. The mouth is in the centre of the upper part; it is not always shaped in the same manner in other anemonies as it is seen here, or at least does not always appear to be so. Fig. A shows a mouth as engraved for another anemone, but which alters or shifts its form every moment. This anemone has 5 rows of limbs. There are 10 in the innermost row: the like number in the 2d: 20 in the 3d: 30 in the 4th: and 80 in the 5th. When the animal is out of the water, and is squeezed, it spurts out water at the mouth and at several of its limbs at the same time; so that it imitates pretty well the play of water-works. When the limbs are drawn in closer together, they give it the look of a flower, especially of an anemone.

Fig. 12 shows an anemone of the same species, turned inside out, as when a purse or stocking is so. A thin transparent membrane, with white stripes, lines the whole inside of the animal; and through it are seen the bowels, part of which hang or come out at the middle. One may observe besides, in this figure, 2 hollows sinking in, which are formed by 2 pretty strong cartilages.

N. B. Dr. Solander being consulted about these sea worms, which are evidently of the class of the actinia, referred the first species, fig. 1—3, to the *Actinia equina*, Linn. Syst. Nat. 1088, 1; the 2d species, fig. 10, 11, and 12, to the *Actinia senilis*, ib. 1088, 2; and the 3d, fig. 13 and 14, to the *Actinia felina*, ib. 1088, 3.

*XXXVIII. Of a New Hygrometer. By M. J. A. De Luc, Citizen of Geneva, F. R. S. p. 404.*

In order to proceed regularly in this investigation, Mr. D. began by examining the essential requisites in a machine intended to measure humidity, which he

\*\* Fig. 10, 11, 12, represent the *Actinia crassicornis* in its different states.

found to be the 3 following: 1st. The settling of a fixed point, from which every measure of the same kind should be taken; such, for instance, as that of boiling water in a thermometer, when the barometer is at a certain height. 2d. Degrees equally determined, or comparable, in different hygrometers, such as are in the thermometer, the scales of Fahrenheit, Delisle, Reaumur, &c. 3d. Constancy in the variations produced by the same differences of humidity.

The result of Mr. D.'s elaborate researches on this subject, was, the adopting for an hygrometer a small tube of ivory, inclosing a portion of quicksilver, which was made to rise and fall in the tube, like a thermometer, by the contracting or widening of the tube, in consequence of more or less moisture in the surrounding medium.

*XXXIX. On the Electric Property of the Torpedo.\* In a Letter from John Walsh, Esq.,† F. R. S., to B. Franklin, Esq., LL. D., F. R. S., &c. p. 461.*

*Letter from Mr. Walsh to Dr. Franklin, dated la Rochelle, 12th July, 1772.*  
 —“ It is with particular satisfaction I make to you my first communication, that the effect of the torpedo appears to be absolutely electrical; by forming its circuit through the same conductors with electricity, for instance, metals and water; and by being intercepted by the same non-conductors, for instance, glass and sealing-wax. I will not at present trouble you with the detail of our experiments, especially as we are daily advancing in them; but only observe, that we have discovered the back and breast of the animal to be in different states of electricity; I mean in particular the upper and lower surfaces of those 2 assemblages of pliant cylinders, of which you have seen engravings in Lorenzini.‡ By the knowledge of this circumstance we have been able to direct his shocks, though they were very small, through a circuit of 4 persons, all feeling them; likewise through a considerable length of wire held by 2 insulated persons, one touching his lower surface, and the other his upper. When the wire was exchanged for glass, or sealing-wax, no effect could be obtained: but as soon as it was resumed, the 2 persons became liable to the shock. These experiments have been varied many ways, and repeated times without number, and they all determined the choice of conductors to be the same in the torpedo as in the Leyden phial. The sensations likewise, occasioned by the one and the other in the human frame, are precisely similar. Not only the shock, but the numbing sensation which the animal sometimes dispenses, expressed in French by the words engourdissement and fourmillement, may be exactly imitated with the

\* *Raja Torpedo*, Linn.

† For this ingenious paper the author was presented with the Copleian medal.

‡ Osservazioni intorno alle torpedini di Stef. Lorenzini 1678. Redi appears to be the first who remarked these singular parts of the torpedo in 1666. Franc. Redi, Exper. Nat.—Orig.

phial, by means of Lane's electrometer; the regulating rod of which, to produce the latter effect, must be brought almost into contact with the prime conductor which joins the phial. We have not yet perceived any spark to accompany the shock, nor the pith balls to be ever affected. Indeed all our trials have been on very feeble subjects, whose shock was seldom sensible beyond the touching finger: I remember but one, of at least 200, that I myself must have received, to have extended above the elbow. Perhaps the Isle of Ré, which we are about to visit, may furnish us with torpedos fresher taken and of more vigour, by which a further insight into these matters may be had. Our experiments have been chiefly in the air, where the animal was more open to our examination than in water. It is a singularity that the torpedo, when insulated, should be able to give us, insulated likewise, 40 or 50 successive shocks, from nearly the same part; and these with little, if any diminution in their force: indeed they were all very minute. Each effort in the animal to give the shock is constantly accompanied with a depression of his eyes, by which even his attempts to give it to non-conductors can be observed. The animal, with respect to the rest of his body, is in a great degree motionless, but not wholly so. You will please to acquaint Dr. Bancroft, of our having thus verified his suspicion concerning the torpedo,\* and make any other communication of this matter you may judge proper. Here I shall be glad to excite, as far as I am able, both electricians and naturalists, to push their inquiries concerning this extraordinary animal, while the summer affords them the opportunity."

*Extracts of a Letter from Mr. Walsh to Dr. Franklin, dated Paris, 27th Aug., 1772.*

—— "I spent a complete week in my experiments at the Isle of Ré, and had there every convenience for prosecuting them to their extent, except that I was restrained by the jealousy of the government from making them where the animal was caught. At my return to La Rochelle, I communicated to the members of the academy of that place, and to many of the principal inhabitants, all that I had observed concerning the torpedo, in the intention of stirring up a spirit of inquiry, both as to its electricity and general economy."

—— "The vigour of the fresh taken torpedos at the Isle of Ré, was not able to force the torpedinal fluid across the minutest tract of air; not from one link of a small chain, suspended freely, to another; not through an almost invisible separation, made by the edge of a penknife, in a slip of tin foil pasted on sealing wax. The spark therefore, of course the attendant snapping noise, was denied to all our attempts to discover it, not only in day light, but in

\* Bancroft's Natural History of Guiana. p. 194.—Orig.

complete darkness. I observed to you, in my last, the singularity of the torpedo being able, when insulated, to give to an insulated person a great number of successive shocks: in this situation I have taken no less than 50 from him in the space of a minute and a half. All our experiments confirmed, that this electricity was condensed, in the instant of its explosion, by a sudden energy of the animal; and as there was no gradual accumulation, nor retention of it, as in the case of charged glass, it is not at all surprizing that no signs of attraction or repulsion were perceived in the pith balls. In short, the effect of the torpedo appears to arise from a compressed elastic fluid, restoring itself to its equilibrium in the same way, and by the same mediums, as the elastic fluid compressed in charged glass. The skin of the animal, bad conductor as it is, seems to be a better conductor of his electricity, than the thinnest plate of elastic air. Notwithstanding the weak spring of the torpedinal electricity, I was able, in the public exhibitions of my experiment at La Rochelle, to convey it through a circuit, formed from one surface of the animal to the other, by 2 long brass wires, and 4 persons, which number, at times, was increased even to 8. The several persons were made to communicate with each other, and the 2 outermost with the wires, by means of water contained in basins, properly disposed between them for the purpose; each person dipping his hands in the nearest basins, connective with his neighbour on either side.

“The effect produced by the torpedo, when in air, appeared, on many repeated experiments, to be about 4 times as strong as when in water.”

A clear and succinct narrative of what passed at one of the public exhibitions, alluded to in the last letter, appeared in the French gazette of the 30th Oct., 1772. As it came from a very respectable quarter, not less so from the private character of the gentleman, than from the public offices he held, I must desire leave of the society to avail myself of such a testimony to the facts I have advanced, by giving a translation of that narrative.

*Extract of a Letter from the Sieur Seignette, Mayor of La Rochelle, and second perpetual Secretary of the Academy of that City, to the publisher of the French Gazette.*

“In the gazette of the 14th Aug., you mentioned the discovery made by Mr. Walsh, member of the parliament of England, and of the r. s. of London. The experiment of which I am going to give you an account, was made in the presence of the academy of this city. A live torpedo was placed on a table. Round another table stood 5 persons insulated. Two brass wires, each 13 feet long, were suspended to the ceiling by silken strings. One of these wires rested by one end on the wet napkin on which the fish lay; the other end was immersed in a basin full of water placed on the 2d table, on which stood 4 other basins likewise full of water. The first person put a finger of one hand in the basin in



which the wire was immersed, and a finger of the other hand in the 2d basin. The 2d person put a finger of one hand in this last basin, and a finger of the other hand in the 3d; and so on successively, till the 5 persons communicated with one another by the water in the basins. In the last basin one end of the second wire was immersed; and with the other end Mr. Walsh touched the back of the torpedo, when the 5 persons felt a commotion which differed in nothing from that of the Leyden experiment, except in the degree of force. Mr. Walsh, who was not in the circle of conduction, received no shock. This experiment was repeated several times, even with 8 persons; and always with the same success. The action of the torpedo is communicated by the same mediums as that of the electric fluid. The bodies which intercept the action of the one, intercept likewise the action of the other. The effects produced by the torpedo resemble in every respect a weak electricity."

This exhibition of the electric powers of the torpedo, before the academy of La Rochelle, was at a meeting, held for the purpose in my apartments, on the 22d July, 1772, and stands registered in the journals of the academy.

The effect of the animal was, in these experiments, transmitted through as great an extent and variety of conductors as almost at any time we had been able to obtain it, and the experiments included nearly all the points, in which its analogy with the effect of the Leyden Phial had been observed. These points were stated to the gentlemen present, as were the circumstances in which the 2 effects appeared to vary. It was likewise represented to them, that our experiments had been almost wholly with the animal in air: that its action in water was a capital desideratum: that indeed all as yet done was little more than opening the door to inquiry: that much remained to be examined by the electrician as well as by the anatomist: that as artificial electricity had thrown light on the natural operation of the torpedo, this might in return, if well considered, throw light on artificial electricity, particularly in those respects in which they now seemed to differ: that for me, I was about to take leave of the animal, as nature had denied it to the British seas; and that the prosecution of these researches rested in a particular manner with them whose shores abounded with it.

The torpedo, on this occasion, dispensed only the distinct, instantaneous stroke, so well known by the name of the electric shock. That protracted but lighter sensation, that torpor or numbness which he at times induces, and from which he takes his name, was not then experienced from the animal; but it was imitated with artificial electricity, and shown to be producible by a quick consecution of minute shocks. This, in the torpedo, may perhaps be effected by the successive discharge of his numerous cylinders, in the nature of a running fire of musketry: the strong single shock may be his general volley. In the continued effect, as well as the instantaneous, his eyes, usually prominent, are withdrawn into their sockets. The same experiments, performed with the same

torpedos, were on the 2 succeeding days repeated before numerous companies of the principal inhabitants of La Rochelle. Besides the pleasure of gratifying the curiosity of such as entertained any on the subject, and the desire I had to excite a prosecution of the inquiry, I certainly wished to give all possible notoriety to facts, which might otherwise be deemed improbable, perhaps by some of the first rank in science. Great authorities had given a sanction to other solutions of the phenomena of the torpedo; and even the electrician might not readily listen to assertions, which seemed, in some respects, to combat the general principles of electricity. I had reason to make such conclusions from different conversations I had held on the subject with eminent persons, both at London and Paris. It is but justice to say, that of all in that class you gave me the greatest encouragement to look for success in this research, and even assisted me in forming hypotheses, how the torpedo, supposed to be endued with electric properties, might use them in so conducting an element as water.

After generally recommending to others an examination of the electric powers of these animals when acting in water, I determined, before I took my final leave of them, to make some further experiments myself with that particular view; since, notwithstanding the familiarity in which we may be said to have lived with them for near a month, we had never detected them in the immediate exercise of their electric faculties against other fish, confined with them in the same water, either in the circumstance of attacking their prey, or defending themselves from annoyance: and yet that they possessed such a power, and exercised it in a state of liberty, could not be doubted.

A large torpedo, very liberal of his shocks, being held with both hands by his electric organs above and below, was briskly plunged into water to the depth of a foot, and instantly raised an equal height into air; and was thus continually plunged and raised, as quick as possible, for the space of a minute. In the instant his lower surface touched the water in his descent, he always gave a violent shock, and another still more violent in the instant of quitting the water in his ascent; both which shocks, but particularly the last, were accompanied with a writhing in his body, as if meant to force an escape: besides these 2 shocks from the surface of the water, which may yet be considered as delivered in the air, he constantly gave at least 2, when wholly in the air, and constantly 1, and sometimes 2, when wholly in the water. The shock in water appeared, as far as sensation could decide, not to have near a 4th of the force of those at the surface of the water, nor much more than a 4th of those entirely in air. The shocks received in a certain time were not, on this occasion, counted by a watch, as they had been on a former, when 50 were delivered, in a minute and a half, by the animal in an insulated and an unagitated state: but from the quickness, with which the immersions were made, it may be presumed there were full 20 of

these in a minute; whence the number of shocks, in that time, must have amounted to above a hundred. This experiment, therefore, while it discovered the comparative force between a shock in water and one in air, and between a shock delivered with greater exertion on the part of the animal and one with less, seemed to determine, that the charge of his organs with electricity was effected in an instant, as well as the discharge.

The torpedo was then put into a flat basket, open at top, but secured by a net with wide meshes, and, in this confinement, was let down into the water, about a foot below the surface; being there touched, through the meshes, with only a single finger, on one of his electric organs, while the other hand was held at a distance in the water, he gave shocks which were distinctly felt in both hands. The circuit for the passage of the effect being contracted to the finger and thumb of one hand, applied above and below to a single organ, produced a shock, to our sensation, of twice the force of that in the larger circuit by the arms.

The torpedo, still confined in the basket, being raised to within 3 inches of the surface of the water, was there touched with a short iron bolt, which was held, half above and half in the water, by one hand, while the other hand was dipped as before, at a distance in the water; strong shocks, felt in both hands, were thus obtained through the iron. A wet hempen cord being fastened to the iron bolt, was held in the hand above water, while the bolt touched the torpedo; and shocks were obtained through both those substances. A less powerful torpedo, suspended in a small net, being frequently dipped into water and raised again, gave, from the surface of the water, slight shocks through the net to the person holding it.

These experiments in water manifested, that bodies, immersed in that element, might be affected by immediate contact with the torpedo; that the shorter the circuit in which the electricity moved, the greater would be the effect; and that the shock was communicable, from the animal in water, to persons in air, through some substances. How far harpoons and nets, consisting of wood and hemp, could in like circumstances, as it has been frequently asserted, convey the effect, was not so particularly tried as to enable us to confirm it. I mention the omission in the hope that some one may be induced to determine the point by express trial.

We convinced ourselves, on former occasions, that the accurate Kaempfer,\* who so well describes the effect of the torpedo, and happily compares it with lightning was deceived in the circumstance, that it could be avoided by holding in the breath, which we found no more to prevent the shock of the torpedo,

\* Kaempf. Amœn. Exot. 1712, p. 514.—Orig.

when he was disposed to give it, than it would prevent the shock of the Leyden Phial.

Several persons, forming as many distinct circuits, can be affected by one stroke of the animal, as well as when joined in a single circuit. For instance, 4 persons, touching separately his upper and lower surfaces, were all affected; 2 persons likewise, after the electricity had passed through a wire into a basin of water, transmitted it from thence, in 2 distinct channels, as their sensation convinced them, into another basin of water, whence it was conducted, probably in a united state, by a single wire. How much further the effect might be thus divided and subdivided into different channels, was not determined; but it was found to be proportionably weakened by multiplying these circuits, as it had been by extending the single circuit.

Something may be expected to be said of the parts of the animal immediately concerned in producing the electrical effect. The engraving, which accompanies this letter, while it shows the general figure of the torpedo, gives an internal view of his electric organs. The society will, besides, have a full anatomical description of these parts from the ingenious Mr. John Hunter, in a paper he has expressly written on the subject at my request. It would therefore be superfluous for me to say any thing either in regard to their situation or structure.

I have to observe however, that in these double organs resides the electricity of the torpedo; the rest of his body appearing to be no otherwise concerned in his electrical effect, than as conducting it: that they are subject to the will of the animal; but whether, like other double parts so controlable, they are exercised, at times, singly as well as in concert, is difficult to be ascertained by experiment: that their upper and under surfaces are capable, from a state of equilibrium with respect to electricity, of being instantly thrown, by a mere energy, into an opposition of a plus and minus state, like that of the charged phial: that when they are thus charged, the upper surfaces of the two are in the same state of electricity; as are the under surfaces of the two, though in a contrary to that of the upper; for no shock can be obtained by an insulated person touching both organs above, or both below: and that the production of the effects depends solely on an intercourse being made between the opposite surfaces of the organs, whether taken singly or jointly.

All the parts bordering on the organs act, more or less, as conductors, either through their substance or by their superficies. While an insulated person, placing 2 fingers on the same surface of one or both organs, cannot be affected; if he removes one of his fingers to any such contiguous part, he will be liable to a shock: but this shock will not be near, perhaps not half, so violent, as one taken immediately between the opposite surfaces of the organ; which shows the

conduction to be very imperfect. The parts which conduct the best, are the 2 great lateral fins bounding the organs outwardly, and the space lying between the 2 organs inwardly. All below the double transverse cartilages scarcely conduct at all, unless when the fish is just taken out of water and is still wet, the mucus, with which he is lubricated, showing itself, as it dries, to be of an insulating nature.

The organs themselves, when uncharged, appeared to be, not interiorly we might suppose, but rather exteriorly, conductors of a shock. An insulated person touching 2 torpedos, lying near one another on a damp table, with fingers placed, one on the organ of one fish, and another on the organ of the other, was sensible of shocks, sometimes delivered by one fish, and sometimes by the other, as might be discovered by the respective winking of their eyes. That the organs uncharged, served some way or other as conductors, was confirmed with artificial electricity, in passing shocks by them; and in taking sparks from them, when electrified. The electric effect was never perceived by us to be attended with any motion or alteration in the organs themselves, but was frequently accompanied with a little transient agitation along the cartilages which surround both organs: this is not discernible in the plump and turgid state of the animal, while he is fresh and vigorous; but as his force decays, from the relaxation of his muscles, his cartilages appear through the skin, and then the slight action along them is discovered.

May we not from all these premises conclude, that the effect of the torpedo proceeds from a modification of the electric fluid? The torpedo resembles the charged phial in that characteristic point of a reciprocation between its 2 surfaces. Their effects are transmitted by the same mediums; than which there is not perhaps a surer criterion to determine the identity of subtle matter: they besides occasion the same impression on our nerves. Like effects have like causes. But it may be objected, that the effects of the torpedo, and of the charged phial, are not similar in all their circumstances; that the charged phial occasions attractive or repulsive dispositions in neighbouring bodies; and that its discharge is obtained through a portion of air, and is accompanied with light and sound; nothing of which occurs with respect to the torpedo. The inaction of the electricity of the animal in these particulars, while its elastic force is so great as to transmit the effect through an extensive circuit, and in its course to communicate a shock, may be a new phenomenon, but is no ways repugnant to the laws of electricity; for here too, the operations of the animal may be imitated by art.

The same quantity of electric matter, according as it is used in a dense or rare state, will produce the different consequences. For example, a small phial, whose coated surface measures only 6 square inches, will, on being highly

charged, contain a dense electricity capable of forcing a passage through an inch of air, and afford the phenomena of light, sound, attraction, and repulsion. But if the quantity condensed in this phial, be made rare by communicating it to 2 large connected jars, whose coated surfaces shall form together an area 400 times larger than that of the phial (I instance these jars because they are such as I use); it will, thus dilated, yield all the negative phenomena, if I may so call them, of the torpedo; it will not now pass the 100th part of that inch of air, which in its condensed state it sprung through with ease; it will now refuse the minute intersection in the strip of tin foil; the spark and its attendant sound, even the attraction or repulsion of light bodies, will now be wanting; nor will a point, brought however near, if not in contact, be able to draw off the charge: and yet, with this diminished elasticity, the electric matter will, to effect its equilibrium, instantly run through a considerable circuit of different conductors perfectly continuous, and make us sensible of an impulse in its passage.

Let me here remark, that the sagacity of Mr. Cavendish in devising, and his address in executing, electrical experiments, led him the first to experience with artificial electricity, that a shock could be received from a charge which was unable to force a passage through the least space of air. But, after the discovery, that a large area of rare electricity would imitate the effect of the torpedo, it may be inquired, where is this large area to be found in the animal? We here approach to that veil of nature, which man cannot remove. This however we know, that from infinite division of parts infinite surface may arise, and even our gross optics tell us, that those singular organs, so often mentioned, consist, like our electric batteries, of many vessels, call them cylinders or hexagonal prisms, whose superficies taken together furnish a considerable area.

I rejoice in addressing these communications to you. He, who predicted and showed that electricity wings the formidable bolt of the atmosphere, will hear with attention, that in the deep it speeds a humbler bolt, silent, and invisible: He, who analyzed the electrified phial, will hear with pleasure that its laws prevail in animate phials: He, who by reason became an electrician, will hear with reverence of an instinctive electrician, gifted in his birth with a wonderful apparatus, and with the skill to use it.

*Explanation of the Plate of the Male and Female Torpedo, or Electric Ray.*

Pl. 9, fig. 1, is a view of the under surface of the female.—a, An exposure, on flaying off the skin, of the right electric organ, which consists of white pliant columns, in a close, and for the most part hexagonal arrangement, giving the general appearance of a honey comb in miniature. These columns have been sometimes denominated cylinders; but, having no interstices, they are all angular, and chiefly 6 cornered.—b, The skin which covered the organ, showing on its inner side, a hexagonal net work.—c, The nostrils in the form of a crescent.—d, The mouth in a crescent contrary to that of the nostrils, furnished with several rows of very small hooked teeth.—e, The branchial apertures, 5 on each side.—f, The place of the heart.—g, g, g, The place of the 2

anterior transverse cartilages, which, passing one above and the other below the spine, support the diaphragm, and uniting towards their extremities, form on either side a kind of clavicle and scapula. h, h, The outward margin of the great lateral fin.—i, i, Its inner margin, confining with the electric organ.—k, The articulation of the great lateral fin with the scapula.—l, The abdomen.—m, m, m, The place of the posterior transverse cartilage, which is single, united with the spine, and supports on each side the smaller lateral fins.—n, n, n, n, The 2 smaller lateral fins.—o, The anus.—p, The fin of the tail.

Fig. 2, A view of the upper surface of the female.—a, a, An exposure of the upper part of the right electric organ.—b, The skin which covered the organ.—c, The eyes, prominent and looking horizontally outwards, but capable of being occasionally withdrawn into their sockets.—d, Two circular apertures communicating with the mouth, and furnished each with a membrane, which in air, as well as in water, plays regularly backwards and forwards across the aperture in the office of inspiration.—e, The place of the right branchia.—f, The two fins of the back.—g, g, The place of the anterior transverse cartilages.

Fig. 3, A view of the under surface of the male, whose size, as here represented, is in general smaller than that of the female.—a, a, Two appendices, distinguishing the male species.

*XL. Anatomical Observations on the Torpedo. By John Hunter, F. R. S.\* p. 481.*

I was desired some time since, by Mr. Walsh, whose experiments at La Rochelle had determined the effect of the torpedo to be electrical, to dissect and examine the peculiar organs by which that animal produces so extraordinary an effect. This I have done in several subjects furnished to me by that gentleman. I am now desired by him to lay before the society, the observations I have made; and for the better understanding of them, to present, on his part, a male and female torpedo in spirits; in the latter of which the electric organs are exposed in different views and sections; likewise a copper plate, which he took care to have engraved, exhibiting those organs.

Of the general structure and anatomy of the torpedo I say nothing, since the animal does not differ very materially, excepting in its electric organs, as they have been properly named by Mr. Walsh, from the rest of the rays, of which family it is well known to be. I will only premise, that the torpedo, of which I treat, is about 18 inches long, 12 broad, and, in its central or thickest part, 2 inches thick; which is nearly the size of the female specimen, now presented to the society, as well as of that from which the plate was taken: but where there is any difference in the organ arising from difference in size, notice will be taken of it in this account.

The electric organs of the torpedo are placed on each side of the cranium and gills, reaching from thence to the semicircular cartilages of each great fin, and extending longitudinally from the anterior extremity of the animal to the transverse cartilage, which divides the thorax from the abdomen; and within these

\* Though this paper has been reprinted in Mr. J. H.'s *Observations on the Animal Economy*; yet as being so immediately connected with Mr. Walsh's *Memoir*, it was thought proper to retain it in these Abridgments.

limits they occupy the whole space between the skin of the upper and of the under surfaces: they are thickest at the edges near the centre of the fish, and become gradually thinner towards the extremities. Each electric organ, at its inner longitudinal edge, is unequally hollowed; being exactly fitted to the irregular projections of the cranium and gills. The outer longitudinal edge is a convex elliptic curve. The anterior extremity of each organ, makes the section of a small circle; and the posterior extremity makes nearly a right angle with the inner edge. Each organ is attached to the surrounding parts by a close cellular membrane, and also by short and strong tendinous fibres, which pass directly across, from its outer edge, to the semicircular cartilages.

They are covered, above and below, by the common skin of the animal; under which there is a thin fascia spread over the whole organ. This is composed of fibres, which run longitudinally, or in the direction of the body of the animal: these fibres appear to be perforated in innumerable places; which gives the fascia the appearance of being fasciculated; its edges all around, are closely connected to the skin, and at last appear to be lost, or to degenerate into the common cellular membrane of the skin. Immediately under this, is another membrane, exactly of the same kind, the fibres of which in some measure decussate those of the former, passing from the middle line of the body outwards and backwards. The inner edge of this is lost with the first described; the anterior, outer, and posterior edges, are partly attached to the semicircular cartilages, and partly lost in the common cellular membrane.

This inner fascia appears to be continued into the electric organ, by so many processes, and thereby makes the membranous sides or sheaths of the columns which are presently to be described; and between these processes the fascia covers the end of each column, making the outermost or first partition. Each organ, of the fish under consideration, is about 5 inches in length, and at the anterior end 3 in breadth, though it is but little more than half as broad at the posterior extremity. Each consists wholly of perpendicular columns, reaching from the upper to the under surface of the body, and varying in their lengths, according to the thickness of the parts of the body where they are placed; the longest column being about an inch and a half, the shortest about  $\frac{1}{4}$  of an inch in length, and their diameters about  $\frac{1}{10}$  of an inch.

The figures of the columns are very irregular, varying according to situation and other circumstances. The greatest number of them are either irregular hexagons, or irregular pentagons; but from the irregularity of some of them, it happens that a pretty regular quadrangular column is sometimes formed. Those of the exterior row are either quadrangular or hexagonal; having one side external, 2 lateral, and either 1 or 2 internal. In the 2d row they are mostly pentagons. Their coats are very thin, and seem transparent, closely connected with each other, having a kind of loose network of tendinous fibres, passing



transversely and obliquely, between the columns, and uniting them more firmly together. These are mostly observable where the large trunks of the nerves pass. The columns are also attached by strong inelastic fibres, passing directly from the one to the other.

The number of columns in different torpedos, of the size of that now offered to the society, appeared to be about 470 in each organ; but the number varies according to the size of the fish.\* These columns increase, not only in size, but in number, during the growth of the animal: new ones forming perhaps every year on the exterior edges, as there they are much the smallest. This process may be similar to the formation of new teeth in the human jaw, as it increases. Each column is divided by horizontal partitions, placed over each other, at very small distances, and forming numerous interstices, which appear to contain a fluid. These partitions consist of a very thin membrane, considerably transparent. Their edges appear to be attached to one another, and the whole is attached by a fine cellular membrane to the inside of the columns. They are not totally detached from one another: I have found them adhering, at different places, by blood vessels passing from one to another.

The number of partitions contained in a column of 1 inch in length, of a torpedo which had been preserved in proof spirit, appeared on a careful examination to be 150: and this number in a given length of column, appears to be common to all sizes in the same state of humidity; for by drying them they may be greatly altered: whence it appears probable that the increase in the length of a column, during the growth of the animal, does not enlarge the distance between each partition in proportion to that growth; but that new partitions are formed, and added to the extremity of the column from the fascia.

The partitions are very vascular; the arteries are branches from the veins of the gills, which convey the blood that has received the influence of respiration. They pass along with the nerves to the electric organ, and enter with them; they then ramify, in every direction, into innumerable small branches on the sides of the columns, sending in from the circumference all around, on each partition, small arteries, which ramify and anastomose on it; and passing also from one partition to another, anastomose with the vessels of the adjacent partitions. The veins of the electric organ pass out, close to the nerves, and run between the gills, to the auricle of the heart.

The nerves inserted into each electric organ, arise by 3 very large trunks, from the lateral and posterior part of the brain. The first of these, in its passage outwards, turns round a cartilage of the cranium, and sends a few branches to the first gill, and to the anterior part of the head, and then passes into the organ towards its anterior extremity. The 2d trunk enters the gills between the

\* In a very large torpedo, the number of columns in one electric organ were 1182.—Orig.

1st and 2d openings, and, after furnishing it with small branches, passes into the organ near its middle. The 3d trunk, after leaving the skull, divides into 2 branches, which pass to the electric organ through the gills; one between the 2d and 3d openings, the other between the 3d and 4th, giving small branches to the gill itself. These nerves, having entered the organs, ramify in every direction, between the columns, and send in small branches, on each partition, where they are lost.

The magnitude and the number of the nerves bestowed on these organs, in proportion to their size, must on reflection appear as extraordinary as the phenomena they afford. Nerves are given to parts either for sensation or action. Now if we except the more important senses of seeing, hearing, smelling, and tasting, which do not belong to the electric organs, there is no part, even of the most perfect animal, which, in proportion to its size, is so liberally supplied with nerves; nor do the nerves seem necessary for any sensation which can be supposed to belong to the electric organs. And with respect to action, there is no part of any animal, with which I am acquainted, however strong and constant its natural actions may be, which has so great proportion of nerves. If it be then probable, that those nerves are not necessary for the purposes of sensation, or action, may we not conclude that they are subservient to the formation, collection, or management of the electric fluid; especially as it appears evident, from Mr. Walsh's experiments, that the will of the animal does absolutely control the electric powers of its body; which must depend on the energy of the nerves. How far this may be connected with the power of the nerves in general, or how far it may lead to an explanation of their operations, time and future discoveries alone can fully determine.

#### *An Explanation of the Engraving of the Torpedo.*

Pl. 9, fig. 4, The upper surface of the electric organ.—AA, The common skin of the animal.—B, The inspiratory opening.—C, The eye.—D, The part in which the gills are inclosed.—EEB, The skin dissected off from the electric organ, and turned outwards; the honeycomb appearance on its internal surface corresponding with the upper surface of the organ.—F, The part of the skin which covered the gills, with some ramifications of an excretory duct on it.—GGG, The upper surface of the electric organ, formed by the upper extremities of the perpendicular columns.

Fig. 5, The right electric organ, divided horizontally into nearly 2 equal parts, at the place where the nerves enter; the upper half being turned outwards.—AA, BB, CC, DD, The corresponding parts of the trunks of the nerves, as they emerge from the gills, and ramify in the electric organ.—AA, The 1st or anterior trunk arising just before the gills.—BB, The 2d or middle trunk arising behind the 1st gill.—CC, The anterior branch of the 3d trunk arising behind the 2d gill.—DD, The posterior branch of the 3d trunk arising behind the 3d gill.

Fig. 6, A perpendicular section of the torpedo a little below its inspiratory openings.—AA, The upper surface of the fish.—BB, The muscles of the back, as divided by the section.—C, The medulla spinalis.—D, The oesophagus.—E, The left gill split, to expose the course of a trunk of the nerve through it.—F, The breathing surface of the right gill.—Gg, The fins.—HH, The perpendicular columns which

compose the electric organ, with a representation of their horizontal partitions.—1, One of the trunks of the nervess, with its ramifications.

END OF THE SIXTY-THIRD VOLUME OF THE ORIGINAL.

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*I. Observations on the Solar Spots. By Alexander Wilson, M.D., Prof. of Astronomy, Glasgow. Anno 1774, Vol. LXIV. p. 1.*

Many astronomers of the first note were very early engaged in the inquiry concerning the solar spots. Of all these Schiener and Hevelius deservedly hold the first place, and nothing but the charms of so noble an investigation could have induced them to prosecute their observations with so much assiduity. Scheiner began his in 1624, 14 years after Galileo had first made the discovery. In 1630, he at last published his *Rosa Ursina*, in which we have a detail of his labours during that long interval of time. Hevelius came after Scheiner, and diligently watched the appearances of the spots for 2 years, the result of which application he has given in his *Selenographia* and *Cometographia*.

But notwithstanding these attempts, so worthy of men actuated by a true desire of knowledge, it must be confessed that nothing of moment has been derived from them. If we except a few conclusions concerning the rotation of the sun round his axis, and the inclination of his axis to the plane of the ecliptic, every thing else, which has been inferred from the phenomena of the spots, seems altogether to be matter of conjecture. Hevelius, from his great fondness of the subject, and from a desire to avail himself of that long course of observation, to which he had so patiently submitted, has been led into many speculations concerning the spots and the nature of the sun's body. In his *Cometographia*, p. 360, he furnishes us with a remarkable instance of this, which serves to give us a view of the ideas he came to entertain on these subjects. But all that we there find, however plausible and ingenious, can be regarded only as conjecture. It does not appear, that any who have followed Hevelius have met with more success. Their observations seem not to differ from his in any remarkable circumstance; nor do we find that their inferences from them, though sometimes different, have any better pretensions to the truth. The many strange and variable circumstances of the spots, which were discoverable from a minute observation, still remained unaccountable; and we often find them at a loss in framing any hypothesis, which could fully satisfy the mind concerning them. In process of time astronomers began to withdraw their attention from a subject which remained so dark and perplexing, and for many years all researches of this sort have been in a great measure laid aside. Chance, or a happy concur-

rence of circumstances, has sometimes effected more than could have been expected from the most promising measures: a remark which, it is hoped, will in some degree be found justified in the sequel of this paper. The observations on the solar spots, now to be related, appear to be totally different from any hitherto to be found, and such as seem to open a new and curious field of speculation into the whole of this subject.

Astronomers will remember, that a spot of an extraordinary size appeared on the sun, in Nov. 1769. On the 22d, Dr. W. had a view of the sun through an excellent Gregorian telescope, of 26 inches focus, which magnified 112 times. It was not far from the sun's western limb, and below his equatorial diameter. The atmosphere being very clear, and free from all tremor and undulation, it was pleasant to see the nucleus of the spot, and the shady zone or umbra which surrounded it, so very distinct. Next day he again saw the spot, having its nucleus and umbra very sharply defined. He now found however a remarkable change; for the umbra, which before was equally broad all round the nucleus, appeared much contracted on that part which lay towards the centre of the disc, while the other parts of it remained nearly of their former dimensions. This change of the umbra seemed somewhat extraordinary, as it was the very reverse of what he expected from the motion of the spot towards the limb. But next day, at 10 o'clock, he had another observation, and discovered changes which were still more unexpected. The distance of the spot from the limb was now about 24". By this time the contracted side of the umbra had entirely vanished; and the figure of the nucleus was now remarkably changed, from what it had been the preceding day. This alteration of the figure appeared evidently to have taken place on that side which had now lost the umbra, the breadth of the nucleus being thereby more suddenly impaired than it ought to have been, by the motion of the spot across the disc.

Regarding these circumstances as new, Dr. W. began to consider what might be the cause of them. One of two things seemed necessarily to be the case; either, that they were owing to some physical alteration or wasting of the spot, and of that part of it where the deficiency of the umbra was observed; or else, that they were owing to the nearer approach of the spot to the limb, by the sun's rotation on his axis. The last of these two ideas had no sooner struck him, than he began to suspect that the central part, or nucleus of this spot, was beneath the level of the sun's spherical surface; and that the shady zone or umbra, which surrounded it, might be nothing else but the shelving sides of the luminous matter of the sun, reaching from his surface, in every direction, down to the nucleus: for, on this supposition, he perceived that a just account could be given of the changes of the umbra, and of the figure of the nucleus, above-described. The opinion therefore which he ventured to form, from what he had

seen this day, was, that this spot might probably be a vast excavation in the luminous matter of the sun; the nucleus, commonly so called, being the bottom, and the umbra the shelving sides of the excavation: and that the umbra, next the centre of the disc, though out of view, did still however exist, and was rendered invisible by its present position only; and further, that the sudden alterations, now discernible in the figure of the nucleus, were occasioned by some part of it also being hid, by the interposition of the edge of the excavation, between the nucleus and the eye.

These considerations made him attentively wait its return. At last, on December 11th, he again discovered it, on the opposite side of the disc, it having by that time advanced a little way from the eastern limb, being distant from it  $1' 30''$ . And now he could only perceive 3 sides of the umbra, namely, the upper and under sides, and that towards the limb, which was the side that formerly had vanished. The side towards the centre of the disc was not as yet visible; but he concluded, on the same grounds as formerly, that it was hid from sight, by its averted position only, and that, after the spot had advanced a little farther, it would make its appearance. Accordingly, the next day, at 10 o'clock, it came into view, and he saw it distinctly, though narrower than the other sides. After this, his observations were interrupted by unfavourable weather, till the 17th, when the spot had passed the centre of the disc, the umbra now appearing to surround the nucleus equally. All the foregoing appearances, when taken together, and when duly considered, seem to prove in the most convincing manner, that the nucleus of this spot was considerably beneath the level of the sun's spherical surface.

The next thing was, to think of some means by which he might form an estimate of its depth. At the time of the observation on Dec. 12th, he had remarked that the breadth of the side of the umbra, next the limb, was about  $14''$ ; but, for determining the point in question, it was also requisite to know the inclination of the shelving side of the umbra to the sun's spherical surface. And here it occurred, that in the case of a large spot, this would, in some measure, be deduced from observation. For, at the time when the side of the umbra is just hid, or begins first to come in view, it is evident, that a line joining the eye and its observed edge, or uppermost limit, coincides with the plane of its declivity. By measuring therefore the distance of the edge from the limb, when this change takes place, and by representing it by a projection, the inclination or declivity in some measure may be ascertained. Dr. W. had not an opportunity, in the course of the foregoing observations, to see the spot at the time when either of the sides of the umbra changed. It is however certain, that when the spot came on the disc for the 2d time, this change happened some time in the night between the 11th and 12th of December; and he judged that

the distance of the plane of the umbra, when in a line with the eye, must have been about  $1' 35''$  from the sun's eastern limb; from which we may safely conclude, that the nucleus of the spot was at that time not less than a semidiameter of the earth below the level of the sun's spherical surface, and formed the bottom of an amazing cavity, from the surface downwards, whose other dimensions were of much greater extent.

Being thus persuaded of the depression of this great spot below the surface, he immediately set about examining smaller ones, in order to discover if they were of the same kind. With this view, he began a course of observations; that from them he might either make the inference universal, or limit it, as the phenomena should point out. Dr. W. was not long engaged in this pursuit, before he perceived in them the same changes of their umbrae, which have been described above. This was manifest in spots of any considerable size, when the air was favourable, and the telescope well adjusted for distinct vision. In general, he found that the umbra thus changes, when a spot is about a minute distant from the limb, at a medium. From all these observations, may we not safely conclude, that every spot consisting of a nucleus and surrounding umbra, as defined by Scheiner and Hevelius, is of the same kind with those above described.

In the course of the foregoing observations, Dr. W. had occasion to remark, 5 different times, another extraordinary circumstance of the spots, which he had not seen mentioned by any one who has written on the subject. It consists of changes which seem to arise from a disturbing force, when one spot breaks out in the neighbourhood of another. The first case of this sort which he met with, was on Nov. 9th, 1770, when the umbra of a spot, though a great way from the limb, was deficient towards the right hand, at which side, and very near it, there lay another spot much smaller. In like manner, 2 other spots, which lay very near each other, had each of them that side of its umbra, which faced the other, taken away. But it was remarkable, that 3 days after, the one spot had nearly vanished, when the side of the umbra of the other spot, which faced it, began now to dilate. Another spot had its umbra flattened on opposite sides, by 3 small spots on one hand, and one on the other. Again, 2 other spots had their umbrae deficient, by the intervention of some small spots, that lay between them.

Now it must here be particularly remarked, that though a spot, when undisturbed, will, when near the sun's limb, exhibit the change of the umbra before mentioned; yet it is plain that a case may now and then occur, when this change will be counteracted, by means of the phenomenon just now described. And accordingly, in the course of the observations, he in reality met with 3 cases, when this change did not take place.

Dr. W. is sensible, that it may be thought strange, that none of the observers, who had looked at the solar spots with so much attention, should ever have taken notice of the gradual changes above described. This partly may be accounted for from the following considerations. We have found that conjectures, concerning the nature of the sun, were early indulged in the course of this inquiry. His body was thought to be an immense globe of fire, which was for ever raging with the most fervent heat. Hence the first observers, reflecting on the perpetual generation, changes, and decay of the spots, and that through so wide an extent of his surface, very naturally imagined, that they could consist of nothing but smoke and grosser exhalations, or such transient and perishable materials. This hypothesis had at least the air of being supported by a very plausible analogy. The minds of men being carried away by such prepossessions, it would less readily occur, that successful observations were only to be made, by an accurate and critical attention to those minute changes, which the spots sometimes undergo. But what would still more conduce to this oversight, was the method, which most of them followed, in making their observations. This was by the camera obscura, which both Scheiner and Hevelius often used, and which we find greatly extolled by them, and described at great length in their writings. But spots, when seen in this way, have nothing of that distinctness, which is so remarkable, and so pleasing, when they are viewed directly through a good telescope armed with an helioscope, or glass properly smoked.

It appears then, that the solar spots are immense excavations in the body of the sun; and that what hitherto has been called the nucleus, is the bottom, and what has been called the umbra, the sloping sides of the excavation. It also appears, that the solar matter, at the depth of the nucleus, does not emit light, or emits so little, as to appear dark compared to that resplendent substance at the surface; that this beauteous substance is at the surface most fulgid; and when any of it is seen below the general level, forming the sides of an excavation, that then its lustre is some how impaired, so as to give the appearance of a surrounding umbra. Here our induction ends. To proceed further would be to carry it beyond its true limits, and to intermix with conclusions, which are certain and manifest, the suggestion of hypotheses, which at best are precarious and liable to error. But from what we have now seen, many curious speculations do naturally present themselves. By what mysterious process is it, that those astonishing excavations are at first produced? What is the nature of that shining substance, which is thereby perpetually disturbed? To what are we to ascribe the darkness of the nucleus, and the diminished lustre of the umbra? And what conceptions are we to form of the many strange changes, and at length of the final decay of all these appearances, by which those regions of the sun, that were so hurt and disfigured, again undergo a renovation?

We often find Scheiner and Hevelius mentioning many things concerning the spots, which appeared to them very inexplicable. Hevelius, when speaking of the vast number of spots which break out upon the sun, and of the prodigious size of some of them, admires how from his single body so much matter, exhalations, &c. could be generated, as in any degree to be adequate to so many and so vast phenomena. Now every theory, how ingenious soever, which is founded on a misapprehension of things, is apt to be pressed with many difficulties; and whenever palpable contradictions appear, they may be regarded as proofs of our having fallen into error. It must indeed be acknowledged, that it is very disadvantageous to science, to indulge much in hypotheses, the truth being rarely hit upon in this way, and very often missed. Sometimes however it may not be improper to throw out hints and conjectures, when we can attain to nothing better, provided we are at due pains to distinguish between such, and that real knowledge which we derive, by strict induction, from incontestable principles. The best way therefore, of preserving so proper and necessary a distinction, will be to propose what further remains to be said on this subject, in the form of queries; because, however plausible they may appear, they are at best but matter of conjecture. Hints, when propounded in this way, are freed from the danger of making us rest in any error, while, sooner or later, they may become helps in leading us to a right understanding of the subject.

The queries which Dr. W. makes, are chiefly founded on the following phenomena of the spots, as described by Scheiner and Hevelius. 1. Every spot which has a nucleus, has also an umbra surrounding it. 2. The boundary between the nucleus and umbra is always distinct and well defined. 3. The increase of a spot is gradual, the breadth of the nucleus and umbra dilating at the same time. 4. In like manner the decrease of a spot is gradual, the breadth of the nucleus and umbra contracting at the same time. 5. The exterior boundary of the umbra never consists of sharp angles, but is always curvilinear, how irregular soever the outline of the nucleus may be. 6. The nucleus of a spot, while on the decrease, in many cases changes its figure, by the umbra encroaching irregularly on it; insomuch that, in a small space of time, new encroachments are discernible, by which the boundary, between the nucleus and umbra, is perpetually varying. 7. It often happens, by these encroachments, that the nucleus of a spot is divided into 2 or more nuclei. 8. The nuclei of spots vanish sooner than the umbræ. Many instances of this sort are to be seen in Hevelius' plates, and the same is affirmed by Mr. Derham in the Phil. Trans. 9. Small umbræ are frequently seen without nuclei. 10. An umbra of any considerable size is seldom seen without a nucleus in the middle of it. 11. When a spot, which consisted of a nucleus and umbra, is about to disappear, if it is not succeeded by a facula; or more fulgid appearance; the place which it occupied is soon after



not distinguishable from any other part of the sun's surface. This is certain from the accounts of all observers.

*Queries and Conjectures, tending to explain the above Properties of the Spots.*

When we consider that the solar spots, some of whose properties have just now been enumerated, are so many vast excavations in the luminous substance of the sun, and that, wherever such excavations are found, we always discern dark and obscure parts situated below; is it not reasonable to think, that the great and stupendous body of the sun is made up of two kinds of matter, very different in their qualities; that by far the greater part is solid and dark; and that this immense and dark globe is encompassed with a thin covering of that resplendent substance, from which the sun would seem to derive the whole of his vivifying heat and energy? And will not this hypothesis help to account for many phenomena of the spots in a satisfactory manner? For if a portion of this luminous covering were by any means displaced, so as to expose to our view a part of the internal dark globe, would not this give the appearance of a spot? In this case, would not that part of the dark globe, which is now laid bare, correspond to the nucleus, and the sloping sides of the luminous matter to the umbra? And is not this consonant to that property of a spot mentioned in the first article; for would it not hence follow, that every spot, having a nucleus, should also have an umbra surrounding that nucleus, a natural account being at the same time suggested, for the boundary between the nucleus and umbra being always distinctly defined, as mentioned in the 2d article.

Though we may never have a competent notion of the nature and qualities of this shining and resplendent substance, or of the means by which the excavations in it are formed; we however discover, in their production, the agency of some mighty, though unknown cause, which is there often exerting itself. Though we manifestly behold its effects, yet the mode of its operations may perhaps remain unsearchable. But if we were here to venture a conjecture, might we not suppose, that the luminous matter is so disturbed, and the excavations in it occasioned, by the working of some sort of elastic vapour, generated within the dark globe? And might not this elastic principle, by its expansion, swell into such a volume, as to reach up to the surface of the luminous matter, which would thus be separated and laid aside in all directions? And as there is no regularity in the time of a spot's enlarging, compared to the time of its decreasing, some enlarging quickly, and decreasing slowly, and vice versa, may we not imagine, that this is owing to the duration and quantity of the elastic principle now mentioned? and in general, may we not hence form some idea of the production and subsequent enlargement of a spot, as mentioned in the 3d article?

But to proceed: as we know from experience, that the spots are of a transient

nature, not lasting on the sun for a long space of time, does it not seem reasonable to think, that their gradual decrease, as mentioned in article 4th, is occasioned by the luminous matter encroaching again on that part of the dark globe, which had been uncovered? And from this may we not infer, that the luminous matter gravitates, and is in some degree fluid; for thus would it not have a tendency to flow down, in all directions, and encroach, so as at last to cover the nucleus? And do not these things appear further probable, when we reflect on that uniform inclination, which the sides of the umbra or excavation, have to the external surface of the sun's body? For does not this indicate a fluid sort of matter gradually yielding to the force of gravity? And again, is not this notion further supported, when we consider the property mentioned in the 5th article, namely, that the exterior boundary of the umbra never consists of sharp angles or turnings, but is always curvilinear, and most frequently of a round form: for we know that this boundary is nothing else but the lip of the excavation, which, on supposition that the luminous matter possesses some degree of fluidity, will not be disposed, either in enlarging or contracting, to become irregular by sudden or sharp turnings?

On supposition that the surface of the dark globe of the sun is smooth and level, it may be urged, that the nucleus of a spot, while on the decrease, should, according to the present view of things, always acquire a figure at least nearly circular, and that the luminous matter, continuing to flow down on all sides by an equal gravity, should so encroach on the nucleus, as to make it retain that figure, till at last it be entirely overflowed. But this not being the case, and because it most frequently happens, that the encroachments of the umbra on the nucleus are extremely variable, as mentioned in the 6th article, may we not from this infer, that the surface of the internal dark globe of the sun, is by no means smooth and level, but on the contrary very irregular, for, on this supposition, if for example the area of the nucleus of a great spot were so diversified by mountains and vallies, would not the encroachments of the luminous matter be consequently irregular: and, according as it was more or less retarded or accelerated, at different places, by being contiguous to prominencies or hollows, would not all the alterations in the figure of the decreasing nucleus, how variable soever, be thus plainly accounted for? and because it often happens, that the nucleus of a spot, while on the decrease, is gradually cut in pieces by a luminous zone or zones, which wander across it, as mentioned in the 7th article, does not this look like the gradual flowing in of the luminous matter, as it were, into deep channels, which would thus appear to abound in the surface of the sun's dark body? If we reflect on the irregularities on the surface of this earth, and on the enormous mountains and cavities in the moon, may we not, from such analogy,

imagine, that there may be the like, or much greater, irregularities in the surface of the sun?

Is not the property mentioned in the 8th article, namely, that the nucleus of a spot vanishes sooner than the umbra, also agreeable to the present views? From this state of the phenomenon, we suppose that that part of the sun's dark body, which had been uncovered and exposed to our view, when the spot first broke out, is now again just overflowed by the gradual inundation of the luminous matter. But, after the nucleus thus disappears, may there not however, in many cases, be still left a cavity in the luminous matter, large enough to be perceived? and will not this cavity, so long as it continues, give the appearance of a small undivided umbra? and will not this umbra still be perceivable, till the luminous matter, by continuing to flow in, has filled up the cavity? after which, will not the place of the umbra acquire the same lustre with the rest of the sun's surface, and thus will not all traces of the spot vanish from his body? And do not the particulars mentioned in the 9th, 10th, and 11th articles seem agreeable to what is now said?

Both Scheiner and Hevelius seem to think, that spots sometimes alter their place on the disc, not only by the sun's rotation round his axis, but also by a motion, which they impute to the spots themselves. This Dr. W. could never observe. It is very true, that when a number of small spots lie near one another, there may be from time to time a change of their relative situation; but it is plain that this may proceed entirely from some of them increasing and others diminishing irregularly. But what would further contribute towards forming a judgment of this kind is, the apparent alteration of the relative place, which must arise from the motion across the disc on a spherical surface.

What has been advanced, in the course of the foregoing queries, may perhaps be rendered still more probable, by considering the observations related in the first part of this paper, concerning the changes which are made on the figure of a spot, when another breaks out in its neighbourhood; and which seem to arise from a disturbing force. For, from the cases there laid down, would it not appear, that when a spot is breaking out, the luminous matter is then forced, in all directions, from the nucleus, and is affected much in the same manner, as it would be, were it a fluid matter encompassing the sun's dark body? As to the particular nature and qualities of this luminous matter, we have been sometimes apt to imagine, that it cannot well be any very ponderous fluid, but that it rather must resemble, as to its consistence, a very dense and thick fog, that broods on the surface of the sun's dark body. How far will this idea tend to facilitate our conceptions of the various phenomena of the spots above described?

It has been gathered from many observations, that the time which the spots

take to traverse the whole disc, is nearly equal to the time that they are hid by being on the opposite surface. It is plain, that the time of their appearing on the disc must be some small matter shorter than that of their being hid behind it, on account of our not seeing a complete hemisphere of the sun. But further, it must now be considered, that when a spot just enters the disc, the part which is first visible, is the farthest umbra, by which time the spot has really advanced a whole diameter of itself on the disc. And again, when the same spot goes off the disc, it is evident, that the part which is last visible, is then the farthest umbra, on which account the continuance of the spot on the disc will be shortened by an interval of time, which corresponds nearly to the whole breadth of it. This, as well as the other appearances, described in the first part of this paper, concerning the change of the umbra and figure of the nucleus, when spots approach the limb, are all well illustrated, by making, in a sphere, an excavation similar to what we have described, the bottom of which may be painted black to represent the nucleus, and the sloping sides shaded, if the sphere be of a light colour.

According to the view of things given in the foregoing queries, there would seem to be something very extraordinary in the dark and unignited state of the great internal globe of the sun. Does not this seem to indicate, that the luminous matter which encompasses it derives not its splendour from any intensity of heat? For if this were the case, would not the parts underneath, which would be perpetually in contact with that glowing matter, be heated to such a degree, as to become luminous and bright? At the same time it must be confessed, that though the internal globe was in reality much ignited, yet when any part of it, forming the nucleus of a spot, is exposed to our view, and is seen in competition with a substance of such amazing splendor, it is no wonder that an inferior degree of light should in these circumstances be unperceivable. But from the nature of the thing, does there seem any necessity for thinking, that there prevails there any such raging and fermenting heat, as many have imagined? It is proper here to attend to the distinction between this shining matter of the sun, and the rays of light which proceed from it. It may perhaps be thought, that the re-action of the rays on the matter, at their emission, may be productive of a violent degree of heat. But whoever would urge this argument, in favour of the sun being intensely heated, as arising from the nature of the thing, ought to consider, that all polished bodies are less and less disposed to be heated, by the action of the rays of light, in proportion as their surfaces are more polished, and as their powers of reflection are brought to a greater degree of perfection. And is there not a strong analogy between the re-action of light on matter, in cases where it is reflected, and in cases where it is emitted?

It may perhaps be expected, that in this paper, mention should be made of the

other appearances, that are discernible on the surface of the sun, besides the spots properly so called; viz. the faculæ, luculi, &c. as described by Scheiner and Hevelius. But all these phenomena seem to be so different from any thing above considered, and so unconnected with the present discovery, that little assistance can be brought from that quarter towards a right conception of them. As to the faculæ, or brighter parts of the sun, we are at a loss for their origin. It may in general be remarked, that though we have obtained an experimental proof, that the luminous matter acquires some degree of shade, when forming the sides of an excavation, yet it is uncertain if this be merely the effect of position, and much more so, if any different modification of position could ever dispose it to put on a brighter or more fulgid appearance. Yet, after all, may not these faculæ, &c. depend on some irregularities in the bright surface of the sun? For may not the luminous matter, by being agitated by the same cause to which the spots owe their origin, though in a less degree, have its surface perpetually disturbed, and made irregular, and thus give occasion to a variety of light and shade, sufficient perhaps to produce the phenomena under consideration? And does not this conjecture receive further confirmation, when we consider, that these faculæ, &c. are found only in that zodiac, within which the spots appear, and that they always abound most in the neighbourhood of the spots themselves, or where spots recently have been? For in those undisturbed regions of the sun that lie towards his poles, and where no spots ever appear, and which Scheiner calls the plagæ æquabiles, we never discover any diversity of appearance.

Thus Dr. W. has endeavoured to give a general idea of the production, changes, and decay of the solar spots, considered as excavations in the body of the sun; a thing which seems to be established from the observations described in the first part of this paper. But concerning the nature of that mighty agency, which occasions those amazing commotions in the luminous matter, or concerning the density, visciduity, and other qualities of this matter, or the manner in which it is disturbed in the middle zone only, and not at the polar regions, and many such other questions, he freely confesses, that they far surpass his knowledge.

*II. Astronomical Observations by the Missionaries at Peking. Transmitted to the Supra-cargoes at Canton, by the Rev. Father Louis Cipolla, of the Tribunal of Mathematics, and communicated to the R. S. by the Court of Directors of the East India Company. p. 31.*

*Preface by the Astronomer Royal.*—Most of the following observations appear to have been made with a telescope of 8 feet, to which a micrometer, for measuring differences of right ascension and declination, was occasionally adapted.

There was besides another telescope of 8 feet, consisting of 2 object-glasses, in the manner of Röemer, which might be brought nearer together or separated, in order that the moon's diameter might completely fill a fine reticule, in the focus, divided into 12 equal parts, for measuring the digits eclipsed in lunar eclipses. It is mentioned in the account of the lunar eclipse of Nov. 12, 1761, that the clock was regulated by a transit instrument. It is therefore probable it was regulated in the same manner in all the succeeding observations, which consist of the following particulars:

1. Observations of the last transit of Venus over the sun, viz. of differences of right ascension and declination between Venus and the sun, and the internal and external contact of the limbs at the egress. It is however remarked, that the clock was counted by a person, who was sometimes found to make mistakes.
- 2. The eclipse of the sun, May 25, 1770. The beginning and end were observed, and the lucid parts measured, during the eclipse, with the micrometer.
- 3. The beginning and end of the eclipse of the moon, Oct. 23, 1771.—4. Emersion of Jupiter from occultation by the moon, July 5, 1770.—5. An occultation of Spica Virginis by the moon; the immersion and emersion both observed Jan. 25, 1772.—6. The occultation of a star in Scorpio by the moon; the immersion and emersion both observed.—7. The observation of Venus in the sun's parallel, Jan. 5, 1772, by taking the difference of right ascension and declination of Venus and the sun.—8. The total eclipse of the moon, Nov. 12, 1761. The beginning, total immersion, emersion, and end, were observed by 3 different observers, with telescopes of 5, 7, and 8 feet, in the domestic observatory of the College of the Jesuits; where also all the former observations were made. The same eclipse was also observed at the Royal Observatory at Pekin, 14' west of the Jesuit's College, with a telescope of 8 feet, composed of 2 object-glasses, with a reticule at the focus, divided into 12 equal intervals for measuring the digits eclipses, in the manner of Röemer. It is remarked, that the leaf, in which this observation was recorded, had been lost, and was found again, Oct. 12, 1772; on which account this observation was never transmitted to Europe before.

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The observations themselves are however omitted, being not now of any further use.

*III. The Lunar Eclipse, Oct. 11, 1772, observed at Canton. Communicated by John Blake, Esq., of Parliament-street. p. 46.*

The time being taken only by a watch regulated by the sun the day before, the observation is not much to be depended on.

NEVIL MASKELYNE.

*IV. Experiments on Dying Black. By Mr. James Clegg, of Redivales, near Bury. p. 48.*

Lime having been proved to increase the solvent power of water, on astringent

vegetables, for medical purposes, Mr. C. was desirous of knowing if it would be equally useful in the art of dying black: to this end he made the following experiments.

*Exper. 1.* Four pennyweights of each of the following astringents, viz. galls, sumach, oak bark, bistort root, and logwood, were boiled during 10 minutes, in half a pint of pure river water; on mixing the decoctions with a saturated solution of martial vitriol, in the proportion of  $\frac{1}{4}$  of the solution to  $\frac{3}{4}$  of the decoction, they struck colours differently inclining to blackness, in the following order: viz. oak bark, bistort root, sumach, galls. He then boiled the same weight of all the astringents, in the same quantity of lime water, and on mixing them as above, the colours they produced were inferior to those with plain water, the astringency of the logwood, or whatever gives it the property of striking black with green vitriol, was entirely destroyed; it produced not the least blackness with any quantity of vitriol.

*Exper. 2.* Four pennyweights of each of the astringents above-mentioned, were triturated in plain water, and 4 others in lime water; the measures of water used were equal to those left, after boiling, in the last experiment; and, on being mixed with martial vitriol, as in the last experiment, the colours produced, by this means, were superior to those produced by boiling. Those triturated in lime-water were judged to be the deepest, which agrees with Mr. Henry's experiments; but we must again except the logwood, which gave no colour by trituration, more than by boiling in lime-water.

*Exper. 3.* All the above mixtures having been written with as inks, and exposed 6 months to the air; those boiled in lime-water had failed much; those triturated in lime-water, and in plain water, had faded a little; those boiled in plain water evidently preserved their colour best. On slightly rubbing the faded writings, with a fresh astringent liquor, they recovered their original blackness; by which it appears, that it was the astringent parts of those inks which had failed.

Does it not appear, by these experiments, that, though lime water tends to deepen the colour produced by some astringents and martial vitriol, it by no means adds to the duration of those colours; and as lime-water, either by trituration or coction, entirely destroys the property, in logwood, of striking black with martial vitriol, it can by no means be of service, in the black dye, where logwood is a material ingredient. Does it not also appear, that a slight boiling is preferable to trituration, for the purposes of dying, when a durable colour is wanted?

Having observed a solution of iron, in a vegetable acid, struck a deeper black, on mixture with an astringent, and produced its effects much more expeditiously, than a strong solution of martial vitriol; it occurred, that the iron, being more slightly combined with the vegetable acid than with the vitriolic, made it more

easy for the astringent matter to decompose the former, and produce an ink; if this was the case, he suspected, that lime-water deepened the colour of astringent and chalybeate mixtures, not so much by its action on the astringent, as on the chalybeate, the lime uniting with the superabundant acid, and leaving the iron with so much of the acid, as is necessary for the formation of an ink, to be more easily attached by the astringent matter of the vegetable. But if this theory was well founded, it followed, from analogy, that any substance, which had a greater affinity with the vitriolic acid than iron had, would produce the same effect, in some degree, as lime. To determine this:

*Exper. 4.* He took 2 vessels, containing equal measures of a strong astringent liquor, composed of galls and logwood: into one vessel he put a small quantity of pearl ashes; the other remained as a standard. Pieces of linen and cotton cloth, after maceration in these liquors, were thrown together into a strong solution of copperas; they were soon after taken out, and washed in cold water; when dry, the pieces prepared in ashes were, all of them, much deeper than the others.

He made use of different kinds of pearl and pot-ashes, as well as of many kinds of astringents; the ashes had the same effect, whatever astringent was made use of, and the strongest alkali always produced the deepest colour; and though ashes, used with an astringent, always gave a deeper black, than the same astringent without ashes, yet logwood, which without ashes gave not so deep a colour as galls with them, gave a much deeper black than galls with the same addition. There was a remarkable difference, in this case, between lime and ashes, in their effect on logwood; with lime it gave no blackness; but with ashes, it produced a deeper black than any other astringent he made use of.

Being desirous of trying the duration of colours, produced by astringents, in which different quantities of pearl-ashes had been dissolved;

*Exper. 5.* In 2 pints of river water, he boiled 1 oz. of logwood, during 10 minutes; he then added half an ounce of Aleppo galls, and boiled them together 10 minutes longer; the liquor having stood to cool, was decanted off, and divided into 6 equal quantities. N<sup>o</sup> 1 remained as a standard; into N<sup>o</sup> 2 he put 6 grains of fine pearl-ashes; N<sup>o</sup> 3, 12 grains; N<sup>o</sup> 4, 18 grains; N<sup>o</sup> 5, 24 grains; N<sup>o</sup> 6, 30 grains: to 6 drops of each of these liquors, he added 2 drops of a saturated solution of copperas; N<sup>o</sup> 2 and 3 struck a deep black; N<sup>o</sup> 1 and 4 black, but inferior to 2 and 3; N<sup>o</sup> 5, a brown black; N<sup>o</sup> 6, brown.

From this experiment it appears, that N<sup>o</sup> 5 and 6 were spoiled by an over proportion of ashes. Before seeing experiments, wherein it had been demonstrated, that a quantity of acid enters into the composition of ink, Mr. C. imagined the alkali decomposed the copperas too suddenly, and disengaged the iron faster than the astringent matter could unite with it. But most probably



the alkali neutralized too great a portion of the acid. All these writings having been now exposed 6 months to the air, in N<sup>o</sup> 5 and 6 the blackness is quite destroyed; N<sup>o</sup> 4 is somewhat faded; N<sup>o</sup> 1, 2, 3, remain nearly as they were; N<sup>o</sup> 2 and 3 being still superior to the standard.

*V. Observations on the State of Population in Manchester, and other adjacent Places. By Dr. Percival. p. 54.*

Reprinted in this author's collected works, recently published (1807) in 4 vols. 8vo. by his son.

*VI. Observations on the Bill of Mortality, in Chester, for the year 1772. By Dr. Haygarth. p. 67.*

A writer, of distinguished abilities in political arithmetic, has offered many arguments, which give cause to apprehend that England, in about 70 years, has lost near a quarter of her people. Accurate registers of mortality, with other collateral inquiries, can, with most certainty, confirm or confute this opinion, and determine a question of the most striking importance to our very existence as a nation. The doctrine of annuities for widows, and other persons in old age, the value of reversionary payments, and of assurances on lives, and other important questions in civil society, can only be determined by faithful registers, showing the duration of human life, in various situations of town and country. The slightest view of tables of mortality show, how erroneous every calculation relating to this subject must be, drawn from the London bills, or perhaps those of most other considerable towns, and applied to the inhabitants of this city.

Chester is healthy to an uncommon degree, when compared with towns of the same size. Various circumstances, which contribute to render this place so remarkably salubrious, might be pointed out; but it can here be only observed in general, that this salutary effect may, with great probability, be chiefly attributed to the dry situation, clear air, pure water, and general temperance of the people. In August 1772, the inhabitants of St. Michael's, one of the 9 parishes into which Chester is divided, and situated in the very centre of the city, were numbered with great accuracy: in this parish were 151 families, 127 houses, 618 inhabitants, 246 males, 372 females, 166 married, 41 widows, 21 widowers, and 137 children under 15 years old. Hence the number of persons, never married above 15, is 253. From this account also it appears, that near  $4\frac{1}{2}$  persons dwell in each house; that the proportion of females to males is as 62 to 41, or nearly as 3 to 2; that the widows are to the widowers nearly as 2 to 1; that the number married is little more than one quarter of the inhabitants: the common proportion of married people is about  $\frac{1}{4}$  of the whole. The number of christenings, at St. Michael's, for the last 10 years, are 147, or 14.7 yearly;

the burials, during the same period, are 127, or .127 yearly. Hence the proportion of annual births to inhabitants is nearly as 1 to 42, and burials nearly as 1 to 48½. During 1772, only 9 persons died in this parish; hence the proportion of deaths to the living, this year, is less than 1 in 68. These facts must appear most astonishing to any one who reflects, that in the largest towns, such as London, 1 in 20½ dies annually; and that in towns of a moderate size, as Leeds, 1 in 21½; that in Northampton and Shrewsbury, either of them less than Chester, 1 in 26½ dies yearly. These facts, relating to this parish, are true, beyond a possibility of doubt; and yet they are so very extraordinary, that one cannot, without further inquiries, apply to the whole town, by analogy, the observations which were made on only a small proportion of the inhabitants. However no peculiarity of air, water, or any other obvious circumstance, can be supposed to render this parish more healthy than the rest of the town. How far these facts have been accidental, the following, and other collateral inquiries, will discover.

For the last 8 years, preceding 1772, there have been 385 births, and 375 deaths annually in Chester. The number of deaths this year, excluding those who were killed by the dreadful explosion of gunpowder, is 379; so that, probably, the conclusions drawn from tables, which have been executed with great care and fidelity, will not be liable to any considerable errors; and such errors, by continuing this account for a period of years, will most effectually be corrected. The following observations are offered as a small specimen of the conclusions, that may then with more certainty, be deduced from such a register of mortality. From such bills, which distinguish the ages at which the inhabitants die, it appears, as far as one year's observation may be trusted, that, taking the whole town, 1 in 31.1 dies annually. This proportion, of deaths to the living, is probably too high, because the births, on an average, exceed the burials; a fact, which affords another proof, that the place is uncommonly healthy. Other facts amply confirm this observation.

Half the inhabitants, born in London, die under 2½ years old; in Vienna, under 2; in Manchester, under 5; in Norwich, under 5; in Northampton, under 10; in Chester, this year, above half who died were 20 years old. Of all the children born in this city, 1 in 5½ lives to above 70, and 1 in 15½ attains 80 years of age; whereas in Northampton, only 1 in 21½; in Norwich, 1 in 27; and in London, 1 in 40 lives till 80. In the Hotel Dieu, a large hospital in Paris, above 1 in 5 dies, of all that are admitted; in St. Thomas's and St. Bartholomew's, in London, 1 in 13; in the Chester infirmary, since its first institution in 1755, till 1772 inclusive, only 1 in 25½.

But the annexed table, at one view, shows the comparative state of health, between this and some other towns of different magnitudes. It is curious to

compare, by this table, in the early part of life, the probability that the inhabitants in Chester have, to live longer than in Northampton, Norwich, and especially much longer than in London. But when they have arrived at 70 years old, the chance of living, at all the places, is nearly equal. It is a matter of curiosity, to observe how much longer women live than men. This fact is well established by former observations on this subject, and is confirmed by this register. During the last year 12 widowers have died, and 53 widows; that is above 4 times the number. Between 80 and 90 years old, 2 men and 18 women have died; that is 9 times as many. Above 90 years old, 4 have died, and all women.

The year to which the several ages below have an equal chance to live.

Ages.	Chester.	Northam.	Norwich.	Lond.
0	21½	9½	5	2½
3	55½	43½	43½	34½
5	58½	46½	47	40
10	60	50	52½	44
20	63	53½	55½	47½
40	69	62½	63½	58
50	71½	67½	67	65
60	73½	72½	71½	70½
70	77	78	77	77

*VII. Electrical Experiments. By Mr. Edw. Nairne, of London, Mathematical Instrument-maker. p. 79.*

These experiments were made with an instrument of Mr. Nairne's own workmanship, a description of which is prefixed. The glass cylinder of this machine was 12 inches diameter, and the cylindrical part 19 inches long, exclusive of the necks; the cushion or rubber 14 inches long, and 5 inches broad, supported by 2 wooden springs; which springs were fixed on 2 glass rods, which lie horizontal under the cylinder, and serve to insulate the cushion. The conductor to this machine was 5 feet long, and 12 inches diameter; at the end of it was a short brass rod, with a ball; it was supported on 2 stands, with solid glass rods or pillars. The ball, for receiving the electrical spark from the conductor, was of brass, and fixed to the end of a brass tube, movable in a hole in the top of the receiving stand; from the bottom of this stand a chain passes along the floor, till it is in contact with the chain hanging from the back of the cushion. Mr. N. with this machine has frequently drawn electrical sparks, at the distance of 12, 13, or 13½ inches, from the prime conductor. These were indeed the distances, to which the electrical fire would commonly strike. It would sometimes reach the distance of 14 inches; though but seldom.

Mr. N. used also a small brass conductor, instead of the large one, for charging the batteries, which batteries are composed of 4 boxes, each containing 16 jars of 12 inches high and 4 inches diameter, coated 8 inches high; so that, in the 64 jars, there are very nearly 50 square feet of coated surface. The electrometer is raised, so as to be 4 feet from the bottom, which rests on the jars, to the ball at top. Discharging this battery, through a piece of iron wire (not steel) of ⅓ of an inch diameter, and 45 inches long, it flew about the room in in-

numerable red-hot balls; on examining these balls, they were in general hollow, and seemed to be nothing but scoria. Mr. N. has made a piece of the same wire, of 47 inches long, red-hot, from end to end, so that it separated into several pieces. After this, he took a piece of the fine iron wire before-mentioned, of 6 inches in length, and, to the end of it connected a piece of iron wire  $\frac{1}{4}$  of an inch in diameter, and 48 feet long. Then, on discharging the battery, the electrical fire from the inside passed immediately along the discharging rod to the fine wire, and afterwards had 48 feet to pass, to get to the outside coating of the battery: he then laid another piece, so that the electrical fire passed 48 feet, from the inside of the battery, before it came to the small wire; and again another, so that the electrical fire passed from the inside of the battery 24 feet, before it came to the fine wire, and had 24 feet afterwards to pass, before it could get to the outside coating of the battery; in each case, the 6 inches of the small wire was melted into red-hot balls; and he could not perceive that there was the least difference in the melting of the wire, on its being placed in different parts of the circuit.

Next, he connected to a piece of the same fine iron wire, of 6 inches in length, a piece of the iron wire  $\frac{1}{4}$  of an inch in diameter, and in one continued piece of 274 feet in length. In this arrangement, when the battery was discharged, the electrical fire passed immediately from the discharging rod to the fine wire, and had 274 feet to pass afterwards, to get to the outside coating; then the fine wire was laid next the outside coating of the battery, so that the electrical fire passed 274 feet before it reached it. This experiment was repeated several times, with this difference, that before every discharge of the battery, he shortened the fine wire, till at last there was only half an inch of it connected with the 274 feet of wire; but even that short piece was not made red-hot by the discharge of the 64 jars. The electrical fire, in passing that 274 feet of wire, though it was one entire piece without any joinings, seemed to meet with great resistance, for the explosion from the battery was not so loud, as when a very small electrical bottle is discharged.

Next, he took some silver thread, and made a circuit, of 40 feet, from the inside of the battery to the outside; and at the distance of about 12 feet from the battery, he held the silver thread between his finger and thumb, so that the electrical fire, passing along the thread, passed between them; on discharging the battery, he received a smart shock, particularly in both his ancles, though the thread was held  $3\frac{1}{4}$  feet from the dry floor, on which he stood; by the electrometer, the battery did not appear to be half discharged. He then made a circuit, of 40 feet, with an iron wire  $\frac{1}{4}$  of an inch in diameter, and this was held in the same manner as the silver thread: on discharging the battery through

the iron wire, there was not the least shock felt, though the whole of the battery was discharged, the iron wire of that length conducted it so perfectly.

Mr. N. then tried the effect of the battery on some platina. Several of the grains, or laminæ, were laid on a piece of white wax, so as to make a length of half an inch. On discharging the battery through the platina he found, that not only the surface of some of the laminæ, or grains, had been in fusion, but that part of it was melted in beautiful white spherules, visible to the naked eye.

Another experiment that Mr. N. tried, was on a duck; a chain was fastened to its legs, and, holding it by the wings, the head was brought up to one of the rods of the battery, so that the battery was discharged through it, from the head to the feet: the consequence was, the duck was thrown into violent convulsions, and expired in 2 or 3 minutes. He then took a turkey, and fastened a wire round its neck, and another on its rump, in such manner, that the nearest distance between the wires was along the back bone, thinking the charge of the batteries might pass down the spine, and that the turkey would be made paralytic: but, on discharging the battery, the turkey opened its bill, and died instantly. He then took a cock, and fastened a wire on his rump, and placed one of the balls of the discharging rod on the middle of his back, so that the charge might not pass near his vital parts: the battery being discharged, the body of the cock was violently agitated, for about half a minute, and the head was turned, so that the bill came against its breast; the head and neck however soon recovered, so that it moved its neck, to all appearance, as well as it did before it was struck; but the body was quite motionless, for about 20 minutes; after that it recovered very fast, and, in about 10 minutes more, was able to stand, and walk a little. After this, he put a wire round its neck, in the same manner as on the turkey: the effect was exactly the same; for, on discharging the battery through it, it died instantly. The wire, that conducted the electrical stroke which killed the turkey and cock, was  $\frac{1}{4}$  of an inch in diameter.

The next experiment was on some plants. He discharged the battery through a branch of a balsam, and examined it very attentively immediately after it was struck, but could not perceive, there was the least alteration in the branch, till about 10 or 15 minutes after; and then the upper part of the branch began to droop its head, and continued drooping it, till it hung quite straight down, and in 2 or 3 days entirely withered, though the other part of the plant was very vigorous, and did not appear to be in the least affected; this experiment he repeated, several times, on several balsams, as well as many other plants, and always found the same appearances.

From these experiments we find, that electricity, accumulated to a certain degree, puts an end to vegetable as well as animal life. After having recited

these experiments, Mr. N. mentions a caution, which may be of service to future electricians who may use large batteries. It is, never to discharge their batteries, if it is through a ready conductor, unless the charge passes at least 5 feet from the inside of the battery to the outside; by making use of this precaution, which he learnt from experience, he has discharged the battery near 100 times, and never broke a single jar, by the electrical discharge; before which he was continually breaking them, by discharging the battery in the common method.

There is another experiment, which he mentions, as it probably may give some light in respect to balls, or points, for conductors, for buildings or ships: the apparatus and manner of trying the experiments, is as follows: in fig. 1, pl. 10, A represents the end of the large conductor of the electrical machine; B a brass ball screwed into the end of it, of  $1\frac{2}{3}$  inch diameter; C a small conductor, which was 5 feet 11 inches long, and  $1\frac{1}{8}$  inch diameter; it was made of wood, covered with tin foil, and was insulated, by being supported on a stand, the part D of which was of solid glass. The ball E, at the end of this conductor, was 3 inches diameter, and the ball F  $1\frac{2}{3}$  inch diameter; under this ball F, was a stand G, made of wood covered with tin foil, having a moveable part H, which might be raised higher or lower. On the top of this moveable part was screwed, either a pointed wire, or a wire with a ball  $\frac{3}{4}$  of an inch diameter, and from the bottom of this stand a chain passed along the floor, till it was connected with the chain, which hung from the cushion: he then placed the conductor C, so that the ball E was 4 inches distance from the ball B; and having screwed into the top of the moveable part H, of the stand G, a pointed wire, he moved it till the point was directly under the ball F, at the distance of 3 or 4 inches; and, on exciting the electrical machine, the fire passed from the ball B, to the ball E, and almost at the same instant struck on the point from the ball F. He increased the distance slowly between the point and the ball F, till he found the utmost distance to which it would strike to the point, which was 6 inches; he continued to move the point to 9 inches distance or more; it then was luminous, and the fire continued to strike from the ball B, to the ball E; which showed that the point carried off all the electrical fire from the conductor C, otherwise it would not continue to strike from B to E. He then removed the point, every thing else remaining as before, and in its stead placed a wire, with a ball of  $\frac{3}{4}$  of an inch diameter, at the top of it, at the distance of 3 or 4 inches, directly under the ball F, in the same manner as the point; then, on increasing this distance slowly, the electrical fire was found to strike to the ball at 9 inches, which is half as far again as to the point, and with this remarkable difference, that the quantity of fire was much greater, and

the explosion much stronger and louder, at its striking the ball, than at its striking the point.

It may here be observed, that a point cannot possibly be placed in circumstances more unfavourable than these, to its operation as a point: the body of electric fluid falling on it almost instantaneously, with the stroke from *B* to *E*, so that it had scarcely any measurable time, wherein to act as a point, in diminishing the quantity, before the whole fell on it as a conductor. In the use of points to receive and conduct lightning, they generally act on the electrical atmosphere of a cloud, while the cloud is yet at a distance, diminishing gradually that atmosphere, before the cloud approaches near enough to give the stroke, and thus diminishing the stroke, if not quite preventing it. If the small conductor *c* be placed so as to be in contact with the large conductor *A*, instead of being 4 inches distant, as before, the electrical fire will not strike to the point at any distance whatever; but the point will carry off silently all the electrical fire from the conductors, as fast as the cylinder supplies them, even if the point is placed at the distance of 10 inches or more.

To this machine there was another large conductor, 12 inches diameter, and 5 feet long, which being applied with its points to the back of the cushion, the machine was either negative or positive, only by hanging a chain on either conductor.

*VIII. On the Noxious Quality of the Effluvia of Putrid Marshes. By the Rev. Dr. Priestley to Sir John Pringle. p. 90.*

“ Since the publication of my papers, (says Dr. P.), I have read 2 treatises, written by Dr. Alexander, of Edinburgh, and am exceedingly pleased with the spirit of philosophical inquiry which they discover. They appear to contain many new, curious, and valuable observations; but one of the conclusions, which he draws from his experiments, I am satisfied, from my own observations, is ill founded, and from the nature of it, must be dangerous. I mean his maintaining, that there is nothing to be apprehended from the neighbourhood of putrid marshes. I was particularly surprized to meet with such an opinion as this, in a book inscribed to yourself, who have so clearly explained the great mischief of such a situation, in your excellent treatise on the diseases of the army. On this account I have thought it not improper, to address to you the following observations and experiments, which I think clearly demonstrate the fallacy of Dr. Alexander's reasoning, indisputably establish your doctrine, and indeed justify the apprehensions of all mankind in this case.

I think it probable enough, that putrid matter, as Dr. Alexander has endeavoured to prove, will preserve other substances from putrefaction; because, being

already saturated with the putrid effluvium, they cannot readily take any more; but Dr. Alexander was not aware, that air thus loaded with putrid effluvium, is exceedingly noxious when taken into the lungs. I have lately however had an opportunity of fully ascertaining how very noxious such air is.

Happening to use at Calne, a much larger trough of water, for the purpose of my experiments, than I had done at Leeds, and not having fresh water so near at hand as I had there, I neglected to change it, till it turned black, and became offensive, but by no means to such a degree as to deter me from making use of it. In this state of the water, I observed bubbles of air to rise from it, and especially in one place, to which some shelves, that I had in it, directed them; and having set an inverted glass vessel to catch them, in a few days I collected a considerable quantity of this air, which issued spontaneously from the putrid water; and, putting nitrous air to it, I found that no change of colour or diminution ensued; so that it must have been, in the highest degree, noxious. I repeated the same experiment several times<sup>a</sup> afterwards, and always with the same result.

After this, I had the curiosity to try how wholesome air would be affected by agitation in this water; when, to my real surprize, I found, that after one minute only, a candle would not burn in it; and, after 3 or 4 minutes, it was in the same state with the air which had issued spontaneously from the same water. I also found, that common air, confined in a glass vessel, in contact only with this water, and without any agitation, would not admit a candle to burn in it after 2 days.

These facts certainly demonstrate, that air, which either arises from stagnant and putrid water, or which has been for some time in contact with it, must be very unfit for respiration; and yet Dr. Alexander's opinion is rendered so plausible by his experiments, that it is very possible that many persons may be rendered secure, and thoughtless of danger, in a situation in which they must necessarily breathe it. On this account, I have thought it right to make this communication as early as I conveniently could; and as Dr. Alexander appears to be an ingenuous and benevolent man, I doubt not but he will thank me for it.

That air issuing from water, or rather from the soft earth, or mud, at the bottom of pits containing water, is not always unwholesome, I have also had an opportunity of ascertaining. Taking a walk, about 2 years ago, in the neighbourhood of Wakefield, in Yorkshire, I observed bubbles of air to arise, in remarkably great plenty, from a small pool of water, which, on inquiry, I was informed had been the place where some persons had been boring the ground, in order to find coal. These bubbles of air having excited my curiosity, I presently returned, with a basin, and other vessels proper for my purpose, and having stirred the mud with a long stick, I soon got about a pint of this air; and,



examining it, found it to be good common air; at least a candle burned in it very well. I had not then discovered the method of ascertaining the goodness of common air, by a mixture of nitrous air. Previous to the trial, I had suspected that this air would have been found to be inflammable.

I shall conclude this letter with observing, that I have found a remarkable difference in different kinds of water, with respect to their effect on common air agitated in them, and which I am not yet able to account for. If I agitate common air in the water of a deep well, near my house in Calne, which is hard, but clear and sweet, a candle will not burn in it after 3 minutes. The same is the case with the rain water which I get from the roof of my house. But in distilled water, or the water of a spring well near the house, I must agitate the air about 20 minutes, before it will be so much injured. It may be worth while to make further experiments, with respect to this property of water.

In consequence of using the rain water, and the well water abovementioned, I was very near concluding, contrary to what I have asserted in my printed papers, that common air suffers a decomposition by great rarefaction. For when I had collected a considerable quantity of air, which had been rarefied about 400 times, by an excellent pump made for me by Mr. Smeaton, I always found, that when I filled my receivers with the water above-mentioned, though I did it so gradually as to occasion as little agitation as possible, a candle would not burn in the air that remained in them. But when I used distilled water, or fresh spring water, I undeceived myself.

“ P. s. I cannot help expressing my surprize, that so clear and intelligible an account, of Mr. Smeaton's air pump, should have been before the public so long, as ever since the publication of the 47th vol. of the Philos. Trans., and yet that none of our philosophical instrument makers should attempt the construction. The superiority of this pump, to any that are made on the common plan, is indeed prodigious. Few of them will rarefy more than 100 times, and in a general way not more than 60 or 70 times; whereas this instrument must be in a poor state indeed, if it do not rarefy 200 or 300 times; and when in good order, it will go as far as 1000 times, and sometimes even much farther than that; besides, this instrument is worked with much more ease, than a common air pump, and either exhausts or condenses at pleasure. In short, to a person engaged in philosophical pursuits, this instrument is an invaluable acquisition. I shall have occasion to recite some experiments, which I could not have made, and which indeed I should hardly have dared to attempt, if I had not been possessed of such an air pump as this. It is much to be wished, that some person of spirit in the trade would attempt the construction of an instrument, which would do great credit to himself, as well as be of eminent service to philosophy.”

*IX. Further Proofs of the Insalubrity of Marshy Situations. In a Letter from the Rev. Dr. Price. p. 96.*

“ Dr. Priestley’s paper, on the noxious effects of stagnant waters, read to the R. S., brought to my remembrance, (says Dr. Price), a table, exhibiting the rate of mortality in a parish situated among marshes, which I have seen in Mr. Muret’s Observations, published in the Memoirs of the Economical Society at Bern, for 1766. I have since reviewed this table, and found that it affords a full confirmation of Dr. Priestley’s assertions. This parish is a part of the district of Vaud, belonging to the canton of Bern, in Switzerland; and contains 169 families, and 696 inhabitants. Mr. Muret’s table, of the rate of mortality in it, is formed from a register of the ages at which all died in it for 15 years. With this table he has also given tables, from like registers, of the rates of mortality in 7 small towns; in 36 country parishes and villages; in 16 parishes situated in the Alps; in 12 corn parishes, and in 18 vintage parishes.—From comparing these tables, it appears, that the probabilities of life are highest in the most hilly parts of the province, and lowest in the marshy parish just mentioned. The difference is indeed remarkable, as will appear from the following particulars. One half, of all born in the mountains, live to the age of 47. In the marshy parish, one half live only to the age of 25. In the hills, 1 in 20, of all that are born, live to 80. In the marshy parish, only 1 in 52 reaches this age. In the hills, a person aged 40 has a chance, of 80 to 1, for living a year. In the marshy parish, his chance for living a year is not 30 to 1. In the hills, persons aged 20, 30, and 40, have an even chance for living 41, 33, and 25 years respectively. In the fenny parish, persons, at these ages, have an even chance of living only 30, 23, and 15 years.—In short, it appears, that, though the probabilities of life, in all this country except this one parish, are much higher than in London; yet here, after 30, they are much lower. Before the age of 30, they are indeed higher in this parish; the reason of which must be, that the London air and customs are particularly noxious to children.\*

I am sensible, that observations for only 15 years, in one small parish, do not afford so decisive and ample an authority, in the present case, as there is reason to wish for; and that therefore the perfect exactness, of the particulars I have recited, cannot be depended on.—They are, however, sufficiently near the truth to demonstrate, in general, the unhealthfulness of a marshy situation.”

*X. Of the Culture and Uses of the Son or Sun plant of Hindostan,† with an*

\* In London, one half of all that are born, die under 3 years of age. But this is not peculiar to London. In Berlin the same proportion dies under 3; and at Vienna under 2.—Orig.

† This plant is described by Linnæus, under the name of *Crotalaria juncea*, vid. Spec. Plant.

*Account of the Manner of Manufacturing the Hindostan Paper. By Lieut. Col. Ironside. p. 99.*

This useful plant, Lieut. Col. I. believes, is cultivated all over Hindostan. The seeds are sown in July, before the rains begin; they should be sown near to one another, to make the stem rise higher, more erect, with fewer branches, and to increase the produce. It flowers in October, and is taken up in December. The black ladies use the seeds, reduced to powder and mixed with oil, for their hair, on a supposition that this composition will make their hair grow to a great length, which they are very fond of. From the bark are made all kinds of rope, packing cloths, nets, &c. and from these, when old, most of the paper, in this country, is prepared; for these purposes, the fresh plant is steeped 4 days in water, afterwards tried, and treated as the cannabis for hemp, to which it is so similar when prepared, that Europeans generally suppose it to be the produce of the same plant.

As the substances, producing cloths, ropes, and paper, are few in present use, this plant may perhaps be cultivated with advantage, in some of the British West-India settlements, and in other countries destitute of hemp and flax. It is not improbable, that it may be raised in the warmer climates of Europe, as it ripens here in winter. He could not say, what soils it might refuse; where he had seen it, in the greatest plenty and perfection, had generally been on an earth composed of clay, calcarious grit, and sand. There are other vegetable substances used in Hindostan for the purpose of rope making; one of them is a species of the hibiscus, a description of which he purposed for the subject of another paper: he could scarcely doubt, but that it is only for want of experiments, we had not a greater number of vegetables rendered useful in this manner. The class monadelphia, of Linnæus, promises fair for trials of this kind.

*The Hindostan Method of manufacturing Paper.*

The manufacturer purchases old ropes, clothes, and nets, made from the sun plant, and cuts them into small pieces, macerates them in water, for a few days, generally 5, washes them in the river in a basket, and throws them into a jar of water lodged in the ground; the water is strongly impregnated with a lixivium of sedgi mutti\* 6 parts and quick lime 7 parts. After remaining in this state 8 or 10 days, they are again washed, and while wet, broken into fibres, by a stamping lever, and then exposed to the sun, on a clean terrace, built for this purpose; after which, they are again steeped, in a fresh lixivium, as before.

1004. A figure of it is given by Ehret in Trew's Plant. Select. t. 47; and another in the Hort. Malab. 9, p. 47, t. 26. Both these figures are good.—Orig.

\* Sedgi Mutti is an earth, containing a large portion of fossil alkali. The *raspor* of the antients. It is found in great plenty in this country, and universally used in washing, bleaching, soap-making, and for various other purposes.—Orig.

When they have undergone 3 operations of this kind, they are fit for making coarse brown paper; after 7 or 8 operations, they are prepared for making paper, of a tolerable whiteness.

The rags, thus prepared, are mixed with water in a cistern, to proper consistence; after which it is taken up by a wire frame, to form it into sheets of paper, &c. just in the manner practised in England.

*XI. An Improvement proposed in the Cross Wires of Telescopes. By Dr. Wilson, of Glasgow. p. 105.*

It has been hitherto a desideratum to draw silver wire fine enough for astronomical uses. The means fallen on by Dr. W. of obviating the difficulty, in practice, is extremely simple, and consists in nothing but in flattening the finest wires, which are now drawn. He made the experiment on silver wire, which is marked 500 to the inch. Having prepared a small block of steel, the face of which was made very flat and smooth, a number of the wires were stretched across it, at considerable intervals, by having their ends fastened, by pitch, at each side of the block. This done, he took another block of steel, of the same size, the face of which had been made likewise flat, and the top of it rounded, the better to determine the stroke of the hammer; on applying this, over the wires lying on the first block, which was firmly fixed in a vice, and giving a smart stroke with a hammer of about 5 pounds weight, he found all of them flattened in a very even manner.

That he might have no difficulty of fitting these wires, so flattened, into the telescope, he purposely made the face of the steel blocks a little narrower than the width of the brass ring, in our transit instrument, on which the cross wires are fixed. By this means the wires retained their roundness at both ends, and so were easily fixed across the ring, by the screw pins, when their fine edges regarded the eye. By means also of a simple contrivance, which will readily occur in practice, he made the horizontal wire to go across the others, so as just to touch them. This horizontal wire was a round one, of 500 to the inch, which he purposely used along with the others, that he might form some judgment of the effects of flattened ones, when viewed along with it in the field. He accordingly found a very striking diminution of the visible subtense of these wires, when compared with the round one; and this so considerable, as could not be obtained with round wires, unless they could be drawn to 2 or 3 thousand to the inch.

*XII. The Case of a Patient voiding Stones through a Fistulous Sore in the Loins, without any concomitant Discharge of Urine by the same Passage. By Mr. S. F. Simmons. p. 108.*

This subject, Eleanor Pilcher, was about 52 years of age. About 25 years before she first began to complain of pain in her back, of a difficulty in making water, and of other nephritic symptoms, which gradually increased. Soon after this she began to void gravel with her urine, and to pass several very small stones; and these symptoms continued to return very frequently, and with much severity. About 10 years after the first appearance of these complaints, a swelling came on in the left lumbar region, which, after having been very painful, for a considerable time, suppurated. This wound, which very soon became fistulous, continued open ever after, and constantly afforded an ichorous discharge. It was not till Dec. 1772, 15 years from the appearance of the tumour, that this discharge began to abate, and that the wound, from being perfectly easy, became painful and inflamed. During all this time, the nephritic symptoms had continued to return, without any variation, the urine had constantly afforded a gravelly sediment, and several small stones had passed through the meatus urinarius; but these concretions were now about to take a different course. The pain in the back, which had commonly affected the left side, became much more intense than usual, but was not attended by any of the other symptoms, which had been the usual forerunners of a fit of the gravel. The discharge, from the wound, was suddenly diminished, and the pain and inflammation exceedingly increased, though the urine continued to pass in a healthy quantity, and without difficulty. These complaints continued during 8 days, and then a round and smooth calculus, weighing about 12 grs., was extracted, with some difficulty, from the wound. After that time no gravel was voided with the urine, though no urine ever passed through the wound; and 6 other paroxysms, like that he had described, took place, in which the same symptoms occurred, and which had terminated in a similar manner, so that 7 calculi had passed through the wound, only 2 of which had been preserved, and the least of them weighed 6 grs. During the intervals of these paroxysms, the patient enjoyed a state of ease and health; and the orifice of the wound, soon after the exclusion of a calculus, returned to its usual size, admitting with difficulty a common probe. This case appeared to be a great proof of the powers of nature. The right kidney did not seem to be affected, and as no urine ever passed through the wound, it should seem as if the secretion by the left kidney was destroyed; for, as no gravel was then voided with the urine, the left ureter was probably closed.

The case however, though a very interesting one, is not perfectly singular, for Delecampius relates, that he saw a man who passed several stones through an abscess of the loins, that had become fistulous. And Tulpius, in the 4th book of his *Observationes Medicæ*, gives the history of a patient, who after undergoing much pain, from a nephritic complaint which he inherited from his father, at length passed a stone from the kidneys, externally through the

loins, which occasioned a callous ulcer, through which pus and urine were perpetually flowing. Neither time, nor any of the remedies employed, afforded him any relief; but the passage through the loins closing, and the matter taking a different course, an acute fever was at length brought on, of which the patient died. And the late Mr. Cheselden observes, that he had 3 patients from whom he had extracted small stones, which had made their way from the kidneys to the integuments, and there occasioned an imposthumation. But cases like these, though not perfectly new, seem to deserve to be recorded, as very rare ones, especially when they afford more interesting circumstances than seem hitherto to have occurred.

*XIII. The Disparition of Saturn's Ring, observed by Joseph Varelaz, Lieut. of the Royal Navy of the King of Spain, and Prof. of Mathematics, &c. Translated from the Spanish. p. 112.*

It has been my luck to observe the celebrated phenomenon of the ring of Saturn, which was so much recommended to astronomers, in the gazette of France of July 23. From the 24th of September to the 4th of October, I saw clearly and distinctly the two ansæ of the ring; but with this particular circumstance, that the occidental ansa appeared more strongly illuminated than the oriental. The atmosphere was thicker on the 5th, and I could only see the occidental. The 6th, I thought I could discern some faint remains of the ring; but that might be a deception of my sight, because the atmosphere remained very thick, and the planet could not be seen well terminated. On the 7th, the atmosphere being more transparent, and the heavens clearer than I have ever seen them, I observed the total disparition of the ring; and, having repeated the same observation the following day, I was convinced that this famous phenomenon took place the 6th of the month, in which determination I have all the exactness which can be expected in observations of this kind. The most striking circumstances of this phenomenon were the following; 1. The occidental ansa constantly appeared more bright than the oriental. 2. On the disc of the planet, one could clearly distinguish the line of the shadow projected from the thickness of the ring. 3. On the extremities of this, some luminous points were perceived, which reflected the light more strongly than the others. 4. I did not observe a sensible variation in the apparent diameter of the ring.

*XIV. Of the Gillaroo Trout. By the Hon. Daines Barrington. p. 116.*

“ You will find on the table a Gillaroo trout, as it is termed in Ireland, the peculiarity of which is, that the stomach very much resembles the gizzard of a bird. Since that time I have endeavoured to procure a specimen, with the entrails adhering, and have at last succeeded, the stomach on the table having

been extracted by Mr. Hunter. I do not find that any ichthyologist takes notice of such a part belonging to fish, except Gouan, who says, that the ventricle of some sorts resembles the gizzard of fowls, by being partly fleshy and partly membranous: Gouan however does not specify the species of fish, which hath such a stomach. The poke of the Gillaroo seems to perform the office of a gizzard, because several small snails were found within the present specimen, and I conclude, that this species of food abounds in the lakes which this variety of trout frequents.

By the best information I can procure, they are more common in Lough Corryb, and the lakes of Galway, than the other waters of Ireland: they are also caught in Lough Dern, through which the Shannon runs."

*XV. Account of the Stomach of the Gillaroo Trout. By Mr. H. Watson. p. 121.*

For a more ample dissertation on this subject, with respect to comparative anatomy, the reader is referred to the paper of Mr. Hunter.

*XVI. A Description of a Petrified Stratum, formed from the Waters of Matlock, in Derbyshire. By Matthew Dobson, M.D. p. 124.*

During a short stay at Matlock, this summer, Dr. D. made some observations on the petrifying quality of the waters, and examined a very singular stratum, which has been formed in their course. This stratum he found about 500 yards in length; in several places near 100 yards in breadth; and where thickest from 3 to 4 yards in depth. The manner in which this body of stone has been produced is easily ascertained. Within the memory of some persons now living, the waters of Matlock were not appropriated to the purposes either of bathing or drinking. They issued from near the bottom of the hill which lies to the west immediately behind the present houses, and ran at random down a declivity of about 100 yards, to the river Derwent. In their course they formed large petrified masses, intermingled with great quantities of petrified moss, nuts, leaves, acorns, pieces of wood, and even trunks of trees.

The waters were thus constantly raising obstacles to their own progress, and were frequently therefore forced into new channels; so as by degrees to be extended over a surface of at least 500 yards in length. And by being repeatedly returned into the same channels, a stratum of considerable thickness has been formed. On examining this stratum, some parts are discovered to be extremely hard, and others so soft as easily to be cut. The soft parts however, on exposure to the air, become as hard as flint; and on being struck sound like metal. The reason of this difference in the hardness of different parts, appears to be this: as the waters frequently changed their channels, and repeatedly likewise returned again to the same channels, if, in the intervals, there were any parts considerably

raised, and consequently longer before they were covered with fresh incrustations, these, from a long exposure to the air, would acquire a greater degree of hardness.

Whole houses in the neighbourhood are built of this stone, which they find more durable than any other they meet with; and as it has the excellent property of growing harder, from being exposed, and has likewise many little cavities and interstices, good mortar so insinuates itself into these, as to form a wall as firm as one continued stone. This stratum affords curious and beautifully varied petrifications. Moss exhibits great varieties; for it is evident, that the moss has continued to vegetate, after the roots and lower parts had been penetrated by the stony particles; and thus, stretching itself to a considerable extent, it has in some places been mixed and interwoven with other substances. In some parts snails have been arrested in their sluggish walks, and locked up in the stony concrete. In others, the petrifying matter has shot, in different directions, and formed an intricate kind of net-work. And in others again, there are large masses, which on being broken asunder, are found hollow; and their cavities ornamented with branches of petrification, somewhat resembling coral, but of a darkish white colour, and generally of a rough and granulated surface.

Under the stratum there is, from a foot to a foot and a half, of good soil; and immediately under this lies the limestone rock. The soil is of the same nature with that of the adjoining fields, which form the slope of the hill, and is evidently a continuation of that soil. Any further additions, to this petrified stratum, are now inconsiderable, and in many places none at all; for the two principal springs are confined to their channels, covered from the day, through the greatest part of their course, and are rapid in their motion.

Had proper observations been made on the progress of this stratum, a tolerably exact estimate might have been formed with respect to the time when these waters were first impregnated with their mineral ingredients. From these 2 considerations however, that the stratum is not very thick, and that the soil immediately under it is a continuation of that which lies on the slope of the neighbouring hills, it is probable that many centuries have not been requisite to its production; and consequently that these mineral waters are not of very ancient date.

And if we may rely on an observation, which he had from a plain, inquisitive, and intelligent man on the spot, the source whence these waters derive their impregnation, is in some degree exhausted. This person assured him, from his own experience, that pieces of moss, and other substances, put in the course of the waters, and in the same circumstances as formerly, require more than double the time for their petrification that they did 30 years ago. The stratum therefore, from which the Matlock waters are impregnated, must either be consi-



derably exhausted; or the waters have deviated from their former course, and are now only partially distributed over this stratum.

*XVII. Remarks on the Aurora Borealis. By Mr. Winn. p. 128.*

I believe the observation is new, that the aurora borealis is constantly succeeded by hard southerly, or south-west winds, attended with hazy weather and small rain. I think I am warranted from experience to say constantly; for in 23 instances that have occurred since I first made the observation, it has invariably obtained.

The gale generally commences between 24 and 30 hours after the first appearance of the aurora. More time and observation will probably discover whether the strength of the succeeding gale is proportionate to the splendor and vivacity of the aurora, and the distance of time between them. I only suspect that the more brilliant and active the first is, the sooner will the latter occur, be more violent, but of shorter duration, than when the light is languid and dull.

*XVIII. Experiments concerning the Different Efficacy of Pointed and Blunted Rods, in Securing Buildings against the Stroke of Lightning. By W. Henley, F.R.S. p. 133.*

From an accident which lately happened to the chapel in Tottenham-court road, where a poor man was killed, the gentlemen who have the care of that building were desirous of erecting a proper conductor to prevent such accidents in future; which was done accordingly under Mr. H.'s direction, except 3 points at the top, to which he rather inclined to prefer a single one. On this occasion, he was willing to obtain the best information he could, on the question, whether the preference be due to points or knobs, for the termination of conductors; for which purpose he made the following experiments.

*Exp. 1.* He placed 2 of Mr. Canton's electrometers, A and B, pl. 10, fig. 2, insulated, on stands of sealing-wax, about 7 inches asunder, and as many from the end of a prime conductor, which was 18½ inches long, and 1¼ inch in diameter; and had a ball at each end, 2¼ inches diameter; the diameter of the electrical globe being 9 inches. On the top of the box A, was placed a wire, projecting 3 inches from the end of it, and terminated by a ball ¾ inch in diameter. On the top of the box B, was placed a sharp-pointed wire, projecting also 3 inches from its end. The knob and point were now exactly at the same distance, namely, 7 inches from the end of the conductor. Then, giving the winch 5 or 6 turns, the light cork balls, hanging from the box A, were repelled to the distance of 1 inch from each other; but those hanging from the box B, separated full 2 inches. Then touching the prime conductor with a finger, the balls at A closed, while those at

B remained a full inch asunder. From this experiment, it seems evident how much better adapted a sharp point is to draw off lightning, than a knob of  $\frac{3}{4}$  inch in diameter; and consequently how much more likely to cause it to pass in that conductor, to which it is affixed, rather than in any other part of the building, where it might occasion much damage, as well as endanger the lives of those who might happen to be in it. The following experiments seem to make still more strongly in favour of the same conclusion.

*Exp. 2.* He affixed to the top of a glass-stand a wire,  $\frac{3}{4}$  inch in diameter, terminated at one end by a ball,  $\frac{3}{4}$  inch in diameter; and at the other end by a very sharp point; see fig. 3. Round the middle of this wire was hung a chain 12 inches long. He then charged a bottle, containing 100 square inches of coated surface, and connecting the chain with the coating of the bottle, brought the knob of it very gently towards the ball on the insulated wire, that he might observe precisely at what distance it would be discharged on it; which he found to happen constantly at the distance of half an inch, with a loud and full explosion. Then re-charging the bottle, he brought the knob, in the same gradual manner, towards the point of the insulated wire, to try also at what distance that would be struck; but this in many trials never happened at all. The point, being approached in this gradual manner, always drew off the charge imperceptibly, leaving scarcely a spark in the bottle.

*Exp. 3.* Mr. H. had now recourse to the apparatus known to electricians by the name of the thunder-house, which he thought a nearer resemblance of the operations of nature on these occasions. Having connected a jar, containing 509 square inches of coated surface, with the prime conductor, see fig. 4; he observed, that if it was so charged as to raise the index of the electrometer to 60 degrees, by bringing the ball on the wire of the thunder-house to half an inch distance from that connected with the prime conductor, the jar would be discharged, and the piece in the thunder-house thrown out to a considerable distance. Using a pointed wire for a conductor to the thunder-house, instead of the knob, as in the former experiment, the charge being the same, the jar was discharged silently, though suddenly: and the piece was not thrown out of the thunder-house.

*Exp. 4.* Having made a double circuit to the thunder-house, fig. 5, the 1st by the knob, the 2d by a sharp-pointed wire, at  $1\frac{1}{4}$  inch distance from each other, but of exactly the same height, the charge being the same; though the knob was brought first under that connected with the prime conductor, which was raised half an inch above it, and followed by the point, at  $1\frac{1}{4}$  inch distance, yet no explosion could fall on the knob; the point drew off all the charge silently, and the piece in the thunder-house remained unmoved.

*Exp. 5.* Having insulated the jar, and connected, by chains, with the external

coating, on one side a knob, and on the other side a sharp-pointed wire, both being insulated, and standing 5 inches from each other, fig. 6, he placed a large copper ball, c, 8 inches in diameter (insulated also) so as to stand exactly at half an inch distance both from the knob and the point. The jar being fully charged, he delivered it on the copper ball by the discharging rod, whence it leaped to the knob A, which was  $\frac{3}{4}$  inch in diameter, and the jar was discharged by a loud and full explosion, and the chain was very luminous. He could perceive no light on the chain, which connected the pointed wire B with the coating of the jar.

*Exp. 6.* Mr. H. insulated his 3 largest jars, containing together about 16 square feet of coated surface; fig. 7. From the bottom of these jars projected a wire, terminated by a ball,  $\frac{3}{4}$  inch in diameter; and at the distance of  $1\frac{1}{4}$  inch from it, he placed the insulated ball c; on which he brought down the charge of the 3 jars, by the discharging rod; which leaped from it to the ball in contact with the jars, and discharged them by a loud and full explosion; but the same thing did not happen if he removed the insulated ball only  $\frac{1}{4}$  of an inch farther from the other. He then removed the wire, which was terminated by the ball, from the bottom of the jars; and placed another in its stead, of the same length and diameter, but very nicely tapered to a point, as usual. Then placing the insulated ball c one inch from the point, he brought down the charge of the 3 jars, as before, which flew upon the point, and melted it a little. The jars were discharged with a loud and full explosion. But having removed the ball c to the distance of  $1\frac{1}{4}$  inch from the point, the charge could not strike it; though much of it was presently drawn off silently by the point, as appeared by the falling of the index of the electrometer.

From this experiment, he thinks it seems somewhat more than probable, that a conductor terminated by a ball, of  $\frac{3}{4}$  inch in diameter, would be in danger of a stroke from a highly electrified cloud, at a much greater distance than another with a sharp termination. Indeed he cannot help remarking, how very improbable it appears, that a sharp-pointed conductor should at any time invite or solicit a stroke of lightning. Imagine, if you please, that a large cloud is, by the force of the wind, driven violently towards such a point, and actually strikes on it: yet as the point would act as such, at somewhat more than the striking distance, it seems probable that part of the electricity of the cloud would be drawn off silently, before the actual stroke could be made; and the stroke itself might thereby perhaps be a little lessened.

Mr. H. here inserts what seems to afford a sufficient proof of the truth of this reasoning; viz.

*Extract of a Letter from Capt. Richard Nairne, of the Generous Friends, dated Montreal, June 24, 1773, to Mr. Thos. Marsham, in the Borough.*

• I shall make every observation I can for the good of electricity, and the sa-

tisfaction of my friend Mr. Henley. I put up a longer top-gallant mast the day I arrived at Quebec. The conductor by this means became too short; and my mate still let it hang, without making any addition to it. There was a severe thunder storm that night; but think how pleased I was to find that, from the wetness of the ship's sides, the electricity passed into the water, without the least injury to the ship; but the spark on the point of the conductor, which was very sharp, was so lucid, that my people were very much frightened.'

Since receiving this account of Mr. Nairne's observation, I have been favoured, says Mr. H., with the following remark, by my ingenious and worthy friend, Lieut. Fairlamb, of the artillery; who informs me, that the church of St. Michael, in Charlestown, South Carolina, used to be struck and damaged by lightning, in every 2 or 3 years from its first erection; but in 14 years, that it has been furnished with a pointed conductor, it has never been struck at all. It appears also, that when a stroke of lightning fell on a stable belonging to Wm. Lyttleton, Esq., Governor of South Carolina, and split and threw down 2 of the rafters; yet the dwelling-house, at 20 yards distance, being provided with a conductor, terminated by a sharp point, escaped unhurt. I would here also just remark, that nothing can be more sharply pointed than the weather-fane which terminates the conductor, erected by Mr. Edward Nairne, on one of the pinnacles on the tower of St. Michael's church in Cornhill, which consists of two darts, with a star, having many pointed radii between them; yet in the late thunder-storm, it does not appear that the lightning struck this building; but fell on the key at the top of the spire of St. Peter's church, which is considerably lower than the fane of St. Michael's; and the distance of the two churches is not more than 200 feet. This key is terminated by a thick blunted end: the spire is covered with lead, from the key to the brick tower; and so far the lightning was conducted with safety to the building: nor could I observe, that there had been the least fusion on the metal; but having quitted the lead work, and entered the brick tower, it there did considerable damage, till it reached the leaded roof on the body of the church; whence it seems to have been conducted by the pipes which carry down the rain water, and reach to the bottom of the building, without further damage. Almost at the same instant that this spire was struck, the lightning fell also on a Dutch ship, in the river Thames, lying off the Tower, which had an iron spindle, terminated by a thick blunted end, at her mast-head, and did her much damage. The lightning struck also on the pillar commonly called the obelisk, in the cross road in St. George's fields, Southwark. It likewise struck the chimney of the new Bridewell there, which it threw down to the ridge of that building, which was covered with lead; and then dispersed itself with little damage. The lightning fell also on another chimney at Lambeth; and on a house at the physic-garden near Vauxhall; and,

as before observed, it appears by the best information, nearly at the same time; and in many other places, considerably distant from each other.

I have observed, on another occasion, that if a round ball of metal, 2 inches in diameter, was presented towards the large prime conductor to a good cylinder, at the distance of 2 inches, it would continue to receive such strong sparks, as would give the person who held it a sensible shock in both his legs; but that if the point of a lancet, or a wire 6 inches long, nicely tapered to a point, and tipped with steel, were at the same time held towards the conductor, at the distance of 2 feet, the point would draw off all its electricity silently, and not suffer a spark to pass from it to the ball; and from this experiment I inferred, that a sharp point might probably, in some measure, produce the same effect on a cloud highly charged with electricity, or rather on the electric atmosphere surrounding the cloud; and thus perhaps contribute to lessen a little, if not actually prevent a stroke. I also observed, that if the point of the wire or lancet, was brought nearly into contact with the prime conductor; yet no sensation would be felt in the hand of the operator; and this I imagined was a kind of demonstration, that there could be no danger of inviting a stroke of lightning from a cloud by a sharp-pointed conductor; as it could make no difference in the experiment, whether the point moved towards the large prime conductor, or the conductor moved towards the point. It having however been objected to this experiment, that it was not analogous to the effect of nature operating by a cloud; forasmuch as the cloud being a loose and floating body, it might accede to, and strike on the point with its contents; which the conductor, being a fixed body, was incapable of doing, I made the following experiment.

*Exp. 7.* I procured a bullock's bladder, of the largest size, gilded with leaf copper, and suspended it, by a silken string, at one end of an arm of wood, placed horizontally, and turning freely on the point of a needle; the needle being stuck upright in another piece of wood, inserted in a firm base, and standing in a perpendicular direction to the floor. The bladder was balanced by a leaden weight, at the other end of the wooden arm, as in fig. 8. The apparatus being thus adjusted, I gave the bladder a strong spark from a knob of a charged bottle; when, presenting towards it a brass rod, terminated by a ball, 2 inches in diameter, I observed, that the bladder would come towards it at the distance of 3 inches; it would even come back to it, when swinging in a contrary direction; and when it had got within 1 inch of it, it would throw off its electricity in a full and strong spark: the bladder gave the spark nearly, if not quite, as large as it received it. I then gave it another strong spark as before, when, presenting towards it the pointed wire, I could never perceive that it acceded to that; and when it was brought nearly into contact with the bladder, there was no spark at all, scarcely any sensible quantity of electricity remaining

in it. I repeated the experiments many times, and always with the very same result.

Mr. H. then relates an experiment by Tho. Ronayne, Esq., to the same effect, whence this gentleman draws the following inference: "Now as bodies act at a greater distance, by how much they are more acute, and thereby diminish any known electrical force; and, as in any particular case, the smallness of the pencil, or stroke, depends on the acuteness of the point presented, I cannot avoid giving my suffrage for points, in preference to obtuse bodies."

It may not, says Mr. H., be improper to introduce, in this place, an experiment lately made by Mr. Edward Nairne, in Cornhill; which, though it does not immediately relate to the particular subject of this paper, is a very proper one to demonstrate the utility of metallic conductors in general.

*Mr. Nairne's Experiment.*—He affixes, in a little apparatus, resembling the hulk of a ship, a glass tube, about 8 inches long, and half an inch in diameter, to represent the main-mast. The ends of the tube, which is filled with water, are properly secured by corks; and through each cork a wire is introduced, of such a length as to reach nearly to the middle of the tube, and leave a distance of about half an inch, between the ends of the two: as in a curious experiment of Mr. Lane's, made with small phials. A slight shock, discharged through this apparatus, instantly breaks the tube in pieces, at that part, where the electric matter quits the upper wire, and expands itself in the water, before it reaches the lower one; as the natural electricity has been observed to do in bodies wherein it has met with such an interrupted and broken communication of metal; but Mr. Nairne having fixed, at the top of such a glass tube, and united with the wire of it, a piece of very small harpsichord wire, which was continued to the bottom of it, and there fastened to a regular communication of metal, in contact with the coating of the jars; he discharged through it his 4 batteries united, consisting of 64 jars, containing 50 square feet of coated surface fully charged, when the whole of the small wire was instantly exploded and lost; but the tube remained unhurt: An effect analogous to that of the natural electricity, where, though it has sometimes happened that the conductor, being too small, has been in part destroyed, or much injured by a stroke; yet the building, to which such a conductor has been affixed, has escaped, without receiving the least damage.

Among some very interesting remarks on the effects of lightning, by Professor Winthrop of New Cambridge, which have lately been communicated to me by Dr. Franklin, I find one, on the influence of sharp pointed conductors, so immediately relating to the question under consideration, that no apology will be necessary for introducing it in this place. Dr. Winthrop having given a very curious and exact account of a violent flash of lightning, which fell on and

greatly damaged Hollis-hall, in New Cambridge, observes, that Harvard-hall, being furnished with pointed wires, which wires were at the distance of 160 feet from the chimney of Hollis-hall, on which the lightning-fell, escaped unhurt, though the wires were seen by many to transmit a large quantity of it, which left visible marks on the bricks, where the wires hooked together. This gentleman also observes, that a tree, standing at the distance of 52 feet from a pointed wire, erected on the steeple of a meeting-house, as a conductor for the lightning, had been struck and shivered; but that the meeting-house remained uninjured; and this, he says, is the least distance from such a conductor, so far as he knew, at which any thing had been struck by lightning. It appears therefore very clearly, from these instances, that sharp pointed wires, instead of inviting, and drawing down strokes of lightning, serve rather to prevent them, and that they extend their protecting influence to some distance around them, and ought therefore ever to be used, as the termination of the rods erected on houses, steeples, magazines, masts of ships, &c. in short, on all occasions, where conductors for the lightning may be thought necessary.

I have made many other experiments, says Mr. H., on the different effect of knobs or points, as opposed to insulated electrified bodies; but as they all concur in establishing and confirming the opinions before advanced, it seems unnecessary to mention them; and the more so, as I believe those already recited will be deemed sufficiently decisive without them.

*XIX. Remarks on a Passage in Castillione's Life of Sir Isaac Newton. By John Winthrop. LL.D., at Cambridge, in New England. p. 153.*

There is a passage in Castillione's life of Sir Isaac Newton, prefixed to his edition of the *Opuscula*, in 3 volumes, 4to., published at Lausanne and Geneva in 1744, which appears to be a palpable mistake; and tends to place Sir Isaac Newton in an inferior light to Descartes, in the eyes of foreigners. It is this, p. 32: "*Sæpius se reprehendebat (Neutonus) quòd res merè geometricas algebraicis rationibus tractavisset, et quòd libro suo de Algebra Arithmeticæ Universalis titulum posuisset, melius asserens Cartesium suum de re eadem volumen dixisse Geometriam, ut sic ostenderet has computationes subsidia tantùm esse geometris ad inveniendum.*" The authority he quotes for this, is Dr. Pemberton, in the preface to his *View of Sir Isaac Newton's Philosophy*; but I will venture to say, he has misinterpreted his author. He represents Dr. Pemberton as saying, 1st. That Sir Isaac Newton often censured himself for handling geometrical subjects by algebraic calculations. 2dly. That another thing he often censured himself for, was, his having called his book of algebra by the name of Universal Arithmetic. 3dly. That he commended Descartes, as having done better, in giving the title of Geometry to his treatise on the same subject. The

last two particulars, certainly, and I think the first also, have no foundation in the account Dr. Pemberton has given of this matter. His words are: "I have often heard him (Sir Isaac) censure the handling geometrical subjects by algebraic calculations; and his book of algebra he called by the name of Universal Arithmetic, in opposition to the injudicious title of geometry, which Descartes had given to the treatise, wherein he shows, how the geometer may assist his invention by such kind of computations."—Dr. Pemberton's expression does not at all imply, that Sir Isaac Newton censured himself for handling geometrical subjects by algebraic calculations; the only idea it suggests is, that he censured that way in general, and those who practised it, and that he had his eye particularly on Descartes; and, far from intimating, that he had inconsiderately called his book of algebra by the name of Universal Arithmetic, and afterwards censured himself for doing so, and wished that he had rather called it Geometry, as Descartes did his; it directly affirms, on the contrary, that by express design and choice, he called it Arithmetic, in opposition to Descartes's injudicious title of Geometry.

It is true indeed, that in a following passage, Dr. Pemberton says, "of their (the ancients) taste and form of demonstration, Sir Isaac always professed himself a great admirer: I have heard him even censure himself for not following them yet more closely than he did; and speak with regret of his mistake, at the beginning of his mathematical studies, in applying himself to the works of Descartes, and other algebraic writers, before he had considered the elements of Euclid with that attention, which so excellent a writer deserves."—But the mode of expression here used, is so different from the foregoing, that there can be no doubt, but that it was intended to convey a different meaning. And if in the censure first mentioned, viz. "for handling geometrical subjects by algebraic calculations," Dr. Pemberton had understood that Sir Isaac meant to include himself, this last passage would have been a mere tautology. But this last strongly implies, on the contrary, that Sir Isaac had, in general, endeavoured to follow closely the ancient geometrical form of demonstration, in preference to that by algebraic calculation; which is of modern invention.

There is a remarkable instance of the attention he paid to the distinction between these methods, and of the preference he gave to the former, in his great work of the Principia. Having in lemma 19, and its corollaries, given a concise and elegant solution of a noted geometric problem, he subjoins: "*Atque ita problematis veterum de quatuor lineis ab Euclide incepti, et ab Apollonio continuati, non calculus, sed compositio geometrica, qualem veteres quærebant, in hoc corollario exhibetur.*" That the words, "*non calculus sed compositio geometrica,*" refer to Descartes's prolix algebraic solution of this problem, in his Geometry, p. 25-34, will, I believe, be readily granted by every one acquainted with Sir Isaac Newton's writings.



On the whole, I humbly conceive, that Dr. Pemberton's meaning, in the former passage, might have been better expressed in Latin, as follows: "Sæpius eos reprehendebat, qui res merè geometricas algebraicis rationibus tractavissent; et libro suo de algebra Arithmeticae Universalis titulum ponebat, asserens Cartesium suum de re eadem volumen inscitè dixisse Geometriam, in quo ostendit, quomodo hæ computationes subsidia esse possunt geometris ad inveniendum." Which of these translations does most justly express the sense of the original, may, I suppose, be safely left to the judgment of every person that understands both the languages. I would only add, that this mistake of Castillione must have been owing, either to inadvertence, or to his not being perfectly acquainted with the English language; as he elsewhere appears to have had the highest veneration for Sir Isaac Newton. This mistake may, to some, appear trivial; but, in my apprehension, every circumstance relative to so illustrious a character as that of Sir Isaac Newton, derives importance from it, and ought to be marked with great exactness.

*XX. M. De Luc's Rule for Measuring Heights by the Barometer, reduced to the English Measure of Length, and adapted to Fahrenheit's Thermometer, and other Scales of Heat, and reduced to a more Convenient Expression. By the Astronomer Royal. [Mr. Maskelyne.] p. 158.*

M. De Luc, F. R. S., in his Treatise on the Barometer and Thermometer, has given a rule for the measurement of heights by the barometer, deduced from his experiments, and far more accurate than any published before; since it appears that he could determine heights by it generally to 10 or 15 feet, and that the error seldom, if ever, amounted to double that quantity. This valuable degree of exactness he has obtained principally by detecting the faults of the common barometer, and improving its construction; and by introducing the use of the mercurial thermometer, to accompany that of the barometer. The principal faults, which he found in the common barometers, arose from the repulsion of the quicksilver by the glass tube, from air and moisture admitted into the tube, and from the variations of the density of quicksilver by heat and cold; another very considerable error arose, in calculating heights from the barometer, by not allowing for the changes in the density of the air, whose gravity affords us this measure of heights, owing to heat and cold. The first cause of error, that of the repulsion of the tubes, he remedied, by substituting a syphon barometer instead of the simple upright tube, the repulsion of the two legs of the syphon counteracting itself. The error arising from air and moisture in the tube, he cured by boiling the quicksilver after it was put into the tube, and other precautions. The errors, in the estimation of the heights arising from the changes in the density of the quicksilver, and density of the air by heat and cold, he shows how to correct by allowances depending on two thermometers, one attached to

the frame of the barometer itself, and the other made to be exposed to the open air, to show its degree of heat; which thermometers are to be noted both at the top and bottom of the hill. Lastly, by a great number of experiments made with accurate barometers and thermometers of his own construction, he has deduced a rule for calculating heights of places; the exactness of which he has sufficiently proved by a large table of experiments. But this rule is expressed in French measure, and is adapted either to a thermometer, whose freezing point is 0, and that of boiling water 80, or to thermometers of particular scales. It may therefore be useful to reduce M. de Luc's rule to English measure, and to adapt it to the thermometer of Fahrenheit's scale, which is generally used in this country.

M. de Luc, in the winter season, heated the air of his room to as great degree as he could, and noted the rise of the barometer, owing to the diminution of its density, or specific gravity, by heat; he also noted the height of the thermometer, both before and after the room was heated. Hence he deduced a rule, that when the barometer is at 27 French inches, which was the case in this experiment, an increase of heat, from freezing to that of boiling water, will raise the barometer 6 lines, or  $\frac{1}{4}$  part of the whole. It is easy to see, that when the barometer is higher than 27 French inches, this variation will increase in the same proportion; or will be always  $\frac{1}{4}$  of the height of the barometer; therefore, if the height of the barometer be called  $B$ , the rise of the barometer, for an increase of heat from freezing to boiling water, will be  $\frac{B}{54}$ ; and, as it will be less for a less difference of heat, therefore, if the number of degrees, marked on the thermometer, between freezing and boiling water, he called  $K$ , and the rise of the thermometer from any given point be called  $H$ , the correspondent rise of the barometer will be  $\frac{B}{54} \times \frac{H}{K}$ , by the increase of heat from the given point by the number of degrees  $H$ . If the heat, instead of increasing, was to decrease, then  $H$  would signify so many degrees decrease of heat, and the barometer would sink by  $\frac{B}{54} \times \frac{H}{K}$ . The fixed temperature of heat, to which M. De Luc thought best to reduce his observations of the barometer, is  $\frac{1}{4}$  of the interval from freezing to boiling water above the former point: and if the thermometer was higher than this degree, he subtracted  $\frac{B}{54} \times \frac{H}{K}$ ; if it was lower, he added it to the observed height of the barometer; and thus he obtained the exact height of the barometer, such as it would have been, if the density of its quicksilver had been the same as answers to the fixed degree of temperature. He thus corrected the height of both his barometers (that at the bottom, and that at the top of the hill) for the particular degree of heat, indicated by a thermometer attached to the barometer, at each station; for it might and would commonly happen, that the degree of heat would be different at the two stations. The heights of the barometers, thus corrected, were what he made use of in

his subsequent calculations. Calling these two altitudes of the barometer  $B$  and  $b$ , putting  $\log. B$  and  $\log. b$ , for the logarithms of  $B$  and  $b$ , taking only the first 4 places of figures, after the characteristic, or considering the remaining figures as decimals, and putting  $c$  for the mean height of a thermometer, exposed to the air at top and bottom of the hill, the freezing point being 0, and the point of boiling water at 80, he finds, by his experiments, that the height of the hill will be given in French toises, when  $c$  is  $16\frac{2}{3}$ , by simply taking the difference of the logarithms of the heights of the barometer, or will be equal to  $\log. B - \log. b$ ; and in any other degree of heat, will be greater or less, in proportion as the rarity of the air is greater or less, than in the fixed temperature; or greater or less by  $\frac{c-16\frac{2}{3}}{215}$  part of the whole, for every degree of the thermometer reckoned from the fixed temperature  $16\frac{2}{3}$ ; and consequently the height of the place will be expressed generally in French toises, by this formula,  $\log. B - \log. b + (\log. B - \log. b) \times \frac{c - 16\frac{2}{3}}{215} = (\log. B - \log. b) \times (1 + \frac{c - 16\frac{2}{3}}{215})$ .

To reduce this formula to English measure, and to the scale of Fahrenheit's thermometer, we should first premise some particulars. The French foot is to the English foot, as 1.06575 to 1, as was found by a very accurate experiment: see Phil. Trans., vol 58, for 1768, p. 326; and it is well known, that the point of freezing, on Fahrenheit's thermometer, is at 32, and that of boiling water at 212, or the interval between them 180 degrees. But M. De Luc's point of boiling water 80, was marked when the barometer was at 27 French inches; and it is the custom of our principal English workmen to mark the point of boiling water, 212, on Fahrenheit's thermometer, when the barometer stands at 30 inches, which is equal to 28 inches 1.8 lines French measure; or 13.8 lines higher than M. De Luc's barometer, when he set off the point of boiling water on his thermometers; and it is well known, that the heat of boiling water varies with the weight of the atmosphere: M. De Luc finds, by his experiments, this rule, that an increase of 1 line in the height of the barometer, raises the quicksilver of the thermometer, placed in boiling water, by  $\frac{1}{1134}$  part of the interval between the freezing point and that of boiling water: he afterwards indeed found, that this rule would not answer for such large variations of the barometer, as take place in ascending to very great heights above the earth's surface; but it is accurate enough for any small variation of the barometer, on one side or other of its mean height in these lowest regions of the atmosphere. The change therefore of the boiling point on Fahrenheit's scale, for a change of 1 line in the barometer, will be  $\frac{1}{1134} \times 180 = 0.16$ ; therefore 13.8 lines will cause  $0.16 \times 13.8 = 2.2$  degrees of Fahrenheit's scale; and a thermometer, whose point of boiling water was marked 212, when the barometer stood at 30 English inches = 28 inches 1.8 lines French measure, will, when the barometer descends to 27 French inches, sink 2.2 degrees in boiling water, or to 209.8, or

in round numbers to 210 degrees, which is distant only 178 from 32, the point of freezing. Hence an extent of  $80^\circ$  of M. De Luc's thermometer, answers to an extent of 178 of our Fahrenheit's thermometer; and putting  $F$  for the degrees of this thermometer, corresponding to  $c$  of M. De Luc's, we shall have  $c : F - 32 :: 80 : 178$ , and  $c = (F - 32) \times \frac{80}{178}$ ; which substituted in M. De Luc's formula gives  $(\log. B - \log. b) \times (1 + \frac{c - 16\frac{3}{4}}{215}) = (\log. B - \log. b) \times (1 + \frac{(F-32) \times \frac{80}{178} - 16\frac{3}{4}}{215}) = (\log. B - \log. b) \times (1 + \frac{F - 69.27}{478.38})$ . Where the answer will still come out in French toises, though adapted to Fahrenheit's thermometer. To bring it out in English fathoms, or measure of 6 feet, multiply the above expression by 1.06575, and we shall have in round numbers  $(\log. B - \log. b) \times (1 + \frac{F - 40}{449})$ ; which will express the height between the 2 stations in English fathoms.

In the foregoing expressions,  $B$  and  $b$ , as before said, signify heights of the barometer, at the lower and higher stations, both corrected according to M. de Luc's directions, for the difference of heat between a fixed temperature, (namely  $\frac{1}{2}$  of the interval between freezing and boiling water), and the present heat, indicated by the thermometer attached to the barometer at each station; but it is not necessary to correct both barometers for the effect of heat, but only one for the difference of heat of the two; which will be more convenient also on another account, because the difference of heat, at the two stations, will be generally small, and the correction to reduce one barometer to the heat of the other will consequently be small also; whereas the difference of the present heat, and the fixed temperature, and consequently the correction of both barometers, may be frequently very considerable: this is evident: because if the heat of the barometers, at both stations, was the same, however different from the fixed temperature chosen by M. de Luc, no correction would be necessary; the mercury in the barometer in both stations being expanded in the same proportion, and consequently the difference of the logarithms of its height, at both stations, being the same, as if the heat of both barometers had agreed with that of the fixed temperature. I shall now therefore suppose the upper barometer is to be corrected, to reduce it to the temperature of the lower one, and that  $b$  signifies the height of this barometer, as observed, and not yet corrected; the correction, from what has been said above, calling  $D$  the difference of height of the thermometer attached to the barometer at the two stations, will be  $\pm \frac{Db}{54K}$ , according as the thermometer stands highest at the lower or upper station; and the upper barometer corrected, instead of  $b$ , will be  $b \pm \frac{Db}{54K}$ , which substituted in the formula, gives  $[\log. B - \log. (b \pm \frac{Db}{54K})] \times (1 + \frac{F - 40}{449})$ . But the cor-

rection, on account of the difference of heat of the barometer at the two stations, may be reduced to a still easier expression, in which the variable quantity  $b$ , the height of the upper barometer, shall not appear. The fluxion of a logarithm is to the fluxion of its natural number, as the modulus of the system to the natural number; and 4343 is the modulus of the common logarithms, when the 4 places, next following the characteristic, are taken as whole numbers, instead of decimals, which is meant to be done in the use of the foregoing formula. Therefore  $\frac{db}{54K}$  being very small with respect to  $b$ , we shall have, variation of  $\log. b$ : variation of  $b$  ( $= \frac{db}{54K}$ ) :: 4343 :  $b$  very nearly, and thence variation of  $\log. b = \pm \frac{db}{54K} \times \frac{4343}{b} = \pm \frac{4343D}{54K}$ . Which (putting  $K = 178$ )  $= \pm 0.452D$ . Hence  $\log.(b \pm \frac{db}{54K}) = \log. b \pm 0.452D$ ; which being substituted in the formula above, will give the difference of height, of the two stations, in English fathoms, in a more convenient expression, namely  $(\log. B - \log. b \mp 0.452D) \times (1 + \frac{F-40}{449})$ ; where the upper sign,  $-$ , is to be used, when the thermometer of the barometer is highest at the lower station, and the lower sign,  $+$ , is to be used, when the said thermometer is lowest at the lower station. The first case will be most common; especially where the difference of height of the two stations is considerable. It should also be observed, that when  $F$ , the height of Fahrenheit's thermometer, is less than  $40^\circ$ , then  $+\frac{F-40}{449}$ , becoming negative or subtractive, must be applied in the calculation accordingly.

It may perhaps be convenient to repeat here the meaning of the algebraic terms, used in the foregoing formula, that any person may make use of it, without having occasion to recur to the foregoing investigation.  $B$  signifies the observed altitude of the barometer at the lower station, and  $b$  that at the upper station;  $\log. B$  and  $\log. b$ , signify their logarithms taken out of the common tables, by assuming the first 4 figures next following the characteristic as whole numbers, and considering the 3 remaining figures, to the right hand, as decimals;  $D$  signifies the difference of height of Fahrenheit's thermometer, attached to the barometer at the top and bottom of the hill; and  $F$  signifies the mean of the two heights of Fahrenheit's thermometer, exposed freely for a few minutes to the open air in the shade, at the top and bottom of the hill.

The formula, for the measure of heights, may also be changed; and adapted to thermometers of partiular scales, for the convenience of calculation, as M. de Luc has done; but these scales will be different from his. The thermometer attached to the barometer, had better be divided with the interval between freezing and boiling water, consisting of 81.4 degrees ( $= 180 \times .452$ ) the freezing point may be marked 0, and the point of boiling water will be 81.4; for then,

if the difference of height of this thermometer, at the two stations, be called  $d$ , we shall have  $d = 0.452 \times D$ ; for  $d : D :: 81.4 : 180 :: 0.452 : 1$ ; and the number of degrees expressed by  $d$ , will show immediately the correction for the difference of heat of the two barometers. If the thermometer, designed to show the temperature of the air, be divided with the interval between freezing and boiling water = 200, and the freezing point be marked  $-9$ , and the boiling point  $+191$ , and the heights of this thermometer, at the two stations, be called  $G$  and  $I$ , we shall have  $\frac{F-40}{449} = \frac{G+I}{2 \times 500} = \frac{G+I}{1000}$ . For  $F-40 = F-32-8$ , is the height of Fahrenheit's thermometer, reckoned from 8 degrees above freezing, and  $449 : 500 :: 180 : 200 :: 8 : 9$ , and the fraction  $\frac{F-32-8}{449}$ , if both the numerator and denominator be increased in the ratio of 449 to 500, will become =

$\frac{(F-32-8) \times \frac{500}{449}}{500} = \frac{(F-32)\frac{500}{449}-9}{500} = \frac{G+I}{2 \times 500} = \frac{G+I}{1000}$ , because  $\frac{G+I}{2} + 9 = (F-32) \times \frac{500}{449}$ . Therefore, if the thermometer of the barometer has the freezing point marked 0, and the point of boiling water 81.4, and the difference of its height, at the two stations, be called  $d$ ; and the thermometer for measuring the temperature of the air, be divided with the interval of 200 between the freezing point and that of boiling water, and the first be marked  $-9$ , and the latter  $+191$ , and the degrees shown by this, at the two stations, be called  $G$  and  $I$ ; the formula, that will give the height of the upper station above the lower one, in English fathoms, will be  $(\log. B - \log. b \mp d) \times (1 + \frac{G+I}{1000})$ ; which consequently multiplied by 6, will give the height in English feet. It is to be observed, as before, that  $-d$  or  $+d$  is to be used, according as the thermometer, attached to the barometer, is highest at the lower or upper station; and if  $G$  and  $I$  should happen to fall below 0 of the scale, or to be subtractive, they must be applied accordingly in the calculation.

I shall now add nothing more, but to give the rule for finding heights by the barometer, according to the formulæ delivered above, in common language; first, as adapted to Fahrenheit's thermometer, and next, as adapted to the 2 thermometers of particular scales. Take the difference of the tabular logarithms of the observed heights of the barometer, at the two stations, considering the first 4 figures, exclusive of the index, as whole numbers, and the 3 remaining figures to the right as decimals, and subtract or add  $\frac{1.5.9}{1000}$  of the difference of the altitude of the Fahrenheit's thermometer, attached to the barometer at the two stations, according as it was highest at the lower or upper station; thus you will have the height of the upper station above the lower, in English fathoms nearly: to be corrected, as follows: make this proportion; as 449 is to the difference of the mean altitude of Fahrenheit's thermometer, exposed to the air at

the two stations, from  $40^\circ$ , so is the height of the upper station found nearly, to the correction of the same: which added or subtracted, according as the mean altitude of Fahrenheit's thermometer was higher or lower than  $40^\circ$ , will give the true height of the upper station above the lower, in English fathoms; and multiplied by 6, will give it in English feet.

The same rule, adapted to the thermometers of particular scales, is this: take the difference of the tabular logarithms of the observed heights of the barometer, at the two stations, considering the first 4 figures, exclusive of the index, as whole numbers, and the 3 remaining figures to the right as decimals; and subtract or add the difference of the thermometer, of a particular scale, attached to the barometer, at the two stations, according as it was highest at the lower or upper station, and you will have the height of the upper station above the lower one, in English fathoms, nearly; to be corrected as follows: make this proportion; as 1000 is to the sum of the altitudes of the thermometer of a particular scale, exposed to the air at both stations, so is the height of the upper station above the lower, found nearly, to the correction of the same; which added or subtracted, according as the sum of the altitudes of the thermometers, exposed to the air, is positive or negative, will give the true height of the upper station above the lower in English fathoms; and multiplied by 6, will give it in English feet.

*XXI. Eclipses of Jupiter's Satellites, observed near Quebec. By Sam. Holland, Esq., Surveyor General of Lands for the Northern District of America. p. 171.*

These eclipses of Jupiter's satellites were observed at a house, bearing south  $36^\circ$  west from Quebec, distant from the castle of St. Lewis  $2\frac{1}{4}$  miles, with Dollond's long reflecting telescope.

1770.	Mean time.				
April 19....	14 <sup>h</sup>	5 <sup>m</sup>	21 <sup>s</sup>	.....	immersion of the 2d
May 1....	12	42	32	.....	1st
14....	12	5	30	.....	2d
				.....	not exact to 4" through the thickness of the atmosphere.
21....	13	41	30	.....	2d
24....	12	52	20	.....	1st

} satellite.

*XXII. Observations of the Immersions and Emersions of the Satellites of Jupiter, taken in 1768, by Ensign George Sproule, of H. M. 59th Reg., on the South Point of the Entrance of Gaspee Basin, which bears from Cape Ferrilong, or the Cape forming the Bay to the Northward, N.  $68\frac{1}{4}^\circ$  W. by the true Meridian, distant  $12\frac{1}{4}$  Marine Miles. Communicated by the Astron. Royal. p. 177.*

The latitude of the place of observation, at Gaspee, accurately determined, was  $48^\circ 47' 32''$ . The variation of the needle, by repeated trials different ways, was  $16^\circ 30'$  w.

1768, Jan. 29.. immer. 1.... 13<sup>h</sup> 57<sup>m</sup> 47<sup>s</sup> apparent time.

Mar. 15.. immer. 1.... 14 21 14

16.. immer. 2.... 11 59 7

immer. 3.... 13 30 10

April 9.. emer. 1.... 11 18 26 { N. B. In these 2 emersions the satellites seemed to  
10.. emer. 2.... 11 38 8 { emerge slowly out of the shadow.

25.. emer. 1.... 9 39 40 { This is the best observation, the satellite starting out  
instantaneously.

May 9.. emer. 1.... 13 30 54

12.. emer. 2.... 11 15 43.

*XXIII. Astronomical Observations made by Samuel Holland, Esq., Surveyor General of Lands for the Northern District of North America, for ascertaining the Longitude of several Places in the said District. p. 182.*

Kittery Point, in the province of Main, in Piscataqua harbour.

The latitude by repeated observations,  $43^{\circ} 4' 27''$  N.

Observed, with Dollond's 12-feet refracting telescope, immersion and emersions of  $\mathcal{U}$ 's satellites as follow.

1771. April 11th, an immersion of the 1st, at..... Apparent time.  
15<sup>h</sup> 43<sup>m</sup> 30<sup>s</sup>

27th, same ..... same..... 14 1 43

May 4th, same ..... same..... 15 55 54.

The variation of the compass at this place, is  $7^{\circ} 46'$  west.

Portsmouth, province of New Hampshire, the latitude by repeated observations,  $43^{\circ} 4' 15''$  N.

Observed, with Dollond's 12-feet refracting telescope, immersions and emersions of  $\mathcal{U}$ 's satellites as follow.

		Apparent time.
1772, Sept.	6th, an emersion of the 2d, at .....	11 <sup>h</sup> 9 <sup>m</sup> 20 <sup>s</sup>
	18th, same..... 1st .....	9 42 35
Oct.	11th, same..... same .....	10 5 4
Nov.	3d, same..... same .....	10 23 54
	9th, same..... 2d .....	10 51 39
	12th, same..... 1st .....	6 48 1
	19th, same..... same .....	8 42 44
	23d, immersed entirely, 3d .....	6 8 6
	same began to emerge.. same .....	9 28 14
Dec.	4th, an emersion..... 2d .....	7 50 0
	5th, same..... 1st .....	6 57 44

The variation of the compass at this place is  $7^{\circ} 48'$  west.

*XXIV. Observations of Eclipses of Jupiter's first Satellites made at the Royal Observatory at Greenwich, compared with Observations of the same, made by Samuel Holland, Esq., and others of his Party, in several Parts of North America, and the Longitudes of the Places thence deduced. By the Astronomer Royal. p. 184.*

The result of those comparisons, give the following longitudes of the places of observations.



The meridian of	Place of observation on St. John's island.....	4 <sup>h</sup>	11 <sup>m</sup>	49 <sup>s</sup>	West of Royal Observatory, at Greenwich.
	Louisbourg .....	3	59	57	
	South Point, entrance of Gaspe.....	4	17	50	
	Capt. Holland's house near Quebec.....	4	44	46	
	Kittery Point, province of Main, in Piscataqua } harbour.....	4	42	58	
	Portsmouth, New Hampshire.....	4	42	53	

*XXV. Immersions and Emersions of Jupiter's first Satellite, observed at Jupiter's Inlet, on the Island of Anticosti, North America, by Mr. Thomas Wright, Deputy Surveyor General of Lands for the Northern District of America; and the Longitude of the Place, deduced from Comparison with Observations made at the Royal Observatory at Greenwich, by the Astronomer Royal. p. 190.*

These observations were made at Jupiter's inlet, 2 leagues to the westward of the south-west point of the island of Anticosti, situated at the entrance of the river St. Laurence, in latitude  $49^{\circ} 26'$  north, with a 2-feet reflecting telescope of the late Mr. Short's construction. Mr. Wright's observations of the eclipses of Jupiter's first satellite, corrected, are as follow:

	Apparent time.	Time at Greenw. per Naut. Almanac.	Difference of meridians.
1767, Jan. 17....	immer.. 14 <sup>h</sup> 50 <sup>m</sup> 27 <sup>s</sup> .....	19 <sup>h</sup> 5 <sup>m</sup> 29 <sup>s</sup> .....	4 <sup>h</sup> 15 <sup>m</sup> 2 <sup>s</sup>
Feb. 2....	immer.. 13 2 21.....	17 17 53.....	4 15 32
18....	immer.. 11 19 0.....	15 34 14.....	4 15 14
25....	immer.. 13 13 46.....	17 29 16.....	4 15 30
Mar. 29....	emer. .. 12 10 59.....	16 25 27.....	4 14 28
April 5....	emer. .. 14 7 23.....	18 22 11.....	4 14 48
7....	emer. .. 8 36 19.....	12 51 16.....	4 14 57
14....	emer. .. 10 32 56.....	14 47 48.....	4 14 52
30....	emer. .. 8 54 17.....	13 8 59.....	4 14 42

The mean difference of meridians by the 4 immersions is  $4^h 15^m 19\frac{1}{4}^s$ , and by the 5 emersions is  $4^h 14^m 45\frac{1}{4}^s$ ; both which ought to be corrected, by the help of the nearest observations made at the Royal Observatory at Greenwich. The immersions and emersions observed there, proper to compare with the preceding observations, are these: all observed with a 6-feet reflector, which, I reckon, shows an immersion of the first satellite  $20^s$  later, and an emersion of the same as much sooner, than a 2-feet reflecting telescope.

		Observed at Greenw.	App. time.	App. time per Naut. Almanac.	Correct. of Naut. Alman.
1767,	Jan.	12.... immer. . 11 <sup>h</sup> 41 <sup>m</sup> 41 <sup>s</sup> .....	11 <sup>h</sup> 42 <sup>m</sup> 8 <sup>s</sup> .....	— 0 <sup>m</sup> 27 <sup>s</sup>	
	Feb.	27.... immer. . 11 57 7.....	11 58 6.....	— 0 59	
	Mar.	22.... emer. . 14 28 48.....	14 28 50.....	— 0 2	
	April	9.... emer. . 7 20 1.....	7 20 25.....	— 0 24	
		14.... emer. . 14 47 52.....	14 47 48.....	+ 0 4	
		16.... emer. . 9 16 13.....	9 16 55.....	— 0 42	
		30.... emer. . 13 9 10.....	13 8 59.....	+ 0 11	
	May	9.... emer. . 9 32 26.....	9 33 12.....	— 0 46	

The correction of the Nautical Almanac for a 6-feet reflector, by the mean of the 2 immersions, is  $-43^s$ , which applied to  $4^h 15^m 19\frac{1}{4}^s$ , the longitude of Jupiter's inlet found from immersions, by the help of the Nautical Almanac, gives

$4^h 14^m 36\frac{1}{4}^s$ , the difference of longitude deduced from the immersions. The correction of the Nautical Almanac, by the mean of the 6 emersions, is  $-16\frac{1}{4}''$ , which applied to  $4^h 14^m 45\frac{1}{4}''$ , the longitude of Jupiter's inlet found by the emersions, by the help of the Nautical Almanac, gives  $4^h 14^m 29^s$ , the longitude deduced from the emersions. The mean of these 2 results, found from the immersions and emersions separately, is  $4^h 14^m 33^s$  the proper difference of longitude of Jupiter's inlet west of Greenwich. I have here made no allowance for the difference of power of the 2-feet reflector, used at Jupiter's inlet, and the 6-feet reflector, used at Greenwich; because the mean is taken between the results from the immersions and emersions; which method includes that correction; that is to say, gives the same result whether that correction be made or not. From the foregoing comparisons it should seem that the air is much clearer at the island of Anticosti than at Greenwich, which Mr. Wright confirmed to me, since the immersions give the longitude only  $7\frac{1}{4}^s$  greater than the emersions; which shows that Mr. Wright observed an immersion only  $4^s$  sooner, and an emersion as much later, with a 2-feet reflector, than was done at Greenwich in a 6-feet reflector; though, in an equally good air, this latter telescope would have had the advantage of the former by  $20^s$ , instead of  $4^s$ .

*XXVI. Extract of a Letter from Mr. Humphry Marshall, of West Bradford, in Chester County, Pennsylvania, to Dr. Franklin, sent with Sketches of the Solar Spots, dated May 3, 1773. p. 194.*

The appearances of these spots were not engraven. Mr. M. says he is of opinion that the spots are near the sun's surface, if not closely adhering to it, for these reasons: 1. That their velocities are apparently greatest near the centre, and gradually slower towards each limb. 2. That the shape of the spots varies, according to their position on the several parts of the sun's disc; those that appear broad, and nearly round, when on the middle, seeming, at their first appearance on the eastern limb, but as lines; and, as they advance towards the centre, become oval, then round, and, in their progress to the western limb, appear again as ovals and lines. His other remarks were, that the spots were  $12\frac{1}{4}$  days, and about 2 or 3 hours, in passing; that though some continued visible from one limb to the other, a few would disappear, after having been visible several days; and others divided into parts; that scarcely any spots ever appeared beyond what may be called the polar circles of the sun; and that the same spot never appeared a 2d time, on the eastern limb, at least not in the same form and position.

*XXVII. Account of the House-martin, or Martlet. By the Rev. Gilbert White. p. 196.*

Reprinted in Mr. White's History of Selborne.

*XXVIII. Extract of a Register of the Barometer, Thermometer, and Rain, at Lyndon in Rutland, 1773. By T. Barker, Esq. p. 202.*

This is Mr. Barker's usual annual communication, of the highest, lowest, and mean state, of the barometer and interior and exterior thermometers, for each month in the year. Also the rain of each month, the whole sum being 29 $\frac{1}{4}$  inches.

*XXIX. On certain Receptacles of Air in Birds, which Communicate with the Lungs, and are lodged both among the Fleshy Parts and in the Hollow Bones of those Animals. By John Hunter, F. R. S. p. 205.*

Reprinted with additions in this author's *Observations on the Animal Economy*, 4to., 1786.

*XXX. M. de Luc's Rules, for the Measurement of Heights by the Barometer, Compared with Theory, and Reduced to English Measures of Length, and adapted to Fahrenheit's Scale of the Thermometer: with Tables and Precepts, for Expediting the Practical Application of them. By Samuel Horsley, LL.D. p. 214.*

After the Astronomer Royal's clear and practical paper on the very same subject, in the 20th article preceding, (p. 520) it is quite unnecessary to reprint this very diffuse and elaborate work; and the rather, as other and later accounts of the same thing are to be seen elsewhere, treated in a manner much more simple and perspicuous.

*XXXI. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Apothecaries' Company, for 1773, &c. By Wm. Curtis, clariss. Soc. Pharmaceut. Lond. Soc. Hort. Chelsean. Præfect. et Prælector Botan. p. 302.*

This is the 51st annual presentation, amounting to 2550 plants.

*XXXII. Observations on the Gillaroo Trout, commonly called in Ireland the Gizzard Trout. By John Hunter, F. R. S. p. 310.*

Reprinted in this author's *Observations on the Animal Economy*, before referred to.

*XXXIII. Explication of a most Remarkable Monogram on the Reverse of a very Ancient Quinarius, never before published or explained. By the Rev. John Swinton, B. D., F. R. S. p. 318.*

This piece is a very ancient, or rather an original, quinarius, extremely well preserved. It has on one side a female head in a helmet, with the letter v be-

hind, standing for 5, the number of asses it contains; and on the reverse, Castor and Pollux, or, according to Sig. Olivieri, two Castors, on horseback, with seven stars over each of their helmets, or caps. In the exergue we discover the word ROMA, formed of very ancient characters; and under the belly of one of the horses the monogram, which distinguishes this quinarius from all the other similar pieces that ever fell under my view or observation. Nor have I ever met with it in any author I had occasion to consult or peruse. To me therefore it cannot but appear in the light of an inedited coin.

The Romans first coined silver money, according to Pliny, with whom Livy, in this point, agrees, in the 485th year of the city. Some of the earliest pieces, of which several still remain in the cabinets of the curious and the great, exhibited a female galeated head on one side, as the quinarius now considered; and on the reverse Castor and Pollux, or, as Sig. Olivieri calls them, two Castors, as both these figures are horsemen, such as clearly and distinctly appear on this coin. Therefore, as the letters forming the word ROMA, in the exergue, are antique enough, at least, for the time when silver was first coined at Rome, or 5 years before the commencement of the first Punic war, we may fairly suppose this quinarius to be either coeval with, or, as I rather imagine, a little anterior to the commencement of that war.

The monogram on the reverse of this quinarius, so extremely remarkable for the number of letters it contains, we shall find, on a close and attentive examination, to exhibit the word ROMANORO, the masculine genitive case plural of Romanvs, in the days of C. Duilius and L. Scipio, the son of Barbatus, towards the close of the 5th century of Rome; some time after the completion of which, the Romans converted the last syllable ro into rvm. But to analyse this extraordinary complex character a little more particularly, the first part of it perfectly answers to the word roma, as represented by a monogram on several coins of the Calpurnian family; and the latter part of it is evidently formed of the letters noro, the last of which is apparently included in the head or top of the r. As the masculine plural termination of the genitive case was ro, instead of rvm, in the year of Rome 494, when the inscription mentioning L. Scipio's conquest of Corsica, and reduction of Aleria, seems to have first appeared; it is highly probable, that the piece in question was either coeval with, or a little anterior to, that year. The inscription is as follows:

HONC. OINO. PLOIRVME. CONSENTIONT. R.	<i>Hunc unum plurimi consentiunt Romæ,</i>
DVONORO. OPTVMO. FVISE. VIRO.	<i>Bonorum optimum fuisse virum,</i>
LVCION. SCIPIONE. FILIOS. BARBATI.	<i>Lucium Scipionem. Filius Barbat,</i>
CONSOL. CENSOR. AIDILIS. HIC. FVET.	<i>Consul, Censor, Ædilis, hic fuit.</i>
HIC. CEPIT. CORSICA. ALERIAQVE. VRBE.	<i>Hic cepit Corsicam, Aleriamque Urbem.</i>
DEDET. TEMPESTATEBV8. AIDE. MERETO.	<i>Dedit Tempestatibus ædem merito.</i>

From what has been here laid down it seems highly probable, that this quinarus first appeared about the year of Rome 494, or rather that its first appearance was a little anterior to that year. Which if we admit, it will follow, that the Romans borrowed the monogrammatic way of writing rather from the Etruscans than the Greeks, as I asserted in one of my former papers; with the first of which nations they were perfectly well acquainted, even from the very beginning of their state; whereas they seem to have had little or no intercourse with the other, when the piece in question was coined. It remains, therefore, that what I advanced, in the paper here referred to, is clearly and indubitably true.

With regard to monograms in general, it may not be improper to remark, that they were known and used in several parts of the east, from pretty remote antiquity. They occur on some of the Hebrew, or Samaritan, and Phœnician coins, as well as on the Greek and Roman. I have an exceedingly curious Hebrew, or Samaritan coin, coeval with Simon the Just, prince and high-priest of the Jews, with a monogram on it. That the Phœnicians were not unacquainted with monograms, has been admitted by the learned and ingenious M. Pellerin, and is evinced by one or two of the Phœnician inscriptions on the stones found in the ruins of Citium. That the Arabs likewise anciently used them, on certain occasions, we learn from the ligatures of the Kufic letters, and the inscriptions still remaining on several of the earlier Arabic coins. Nay, they are not disused among the modern Arabs, in their common writing, even at this very day. As for the Greeks, nothing is more common than ligatures, or monograms, on their coins. That the Palmyrenes also had several such ligatures, or complex characters, I have many years since incontestably proved.

With respect to the Romans, nothing is more certain than that combinations of 2, 3, and even 4 elements, formed into one character, not seldom occur on their coins. More extensive or complex ligatures than the monograms of 4 letters on their ancient medals very rarely appear. I have, however, an inedited semissis of the Pompeian family, with the head of Saturn, and behind it the letter s, the mark of the semissis, on one side; and the prow of a ship, over which is a monogram composed of the 5 letters, Q, P, O, M, P.

*XXXIV. Astronomical Observations made at Chislehurst, in Kent, in the Year 1773. By the Rev. F. Wollaston, LL. B., F. R. S. p. 329.*

Mr. W. having the last 2 winters communicated to the R. S. what astronomical observations he had occasionally made in the course of each year, it seems to be a call on him to continue the same now. His instruments and situation are the same as before-described; and the accompanying tables are in the same form as the last year. His clock has been kept going on, without any alteration of any kind;

it is only by long and uninterrupted trials, that any judgment can be formed concerning the cause of errors.

The first is a series of observations on the going of the clock, which gradually gains by the heat of the season, and loses by the cold again. Then a register of the barometer, thermometer, and hygrometer. Next, occultations of stars by the moon. Then eclipses of Jupiter's satellites, after the manner of M. Bailly.

Since the reading of a paper, communicated last year to this society by Dr. Wilson, professor at Glasgow, on the spots of the sun; who mentions some appearances when they approach the limb, which I thought I had now and then observed, says Mr. W.; I have frequently turned my glass that way, as occasion offered, to see whether those appearances were constant, or what might be discovered to confirm the hypothesis laid down in the latter part of that paper. Dr. Wilson, I hope, will excuse me when I say, that the appearance he mentions when the spots approach the sun's limb, as if they were in a cavity on his surface, is not constant. They generally indeed have appeared so to me, I confess. But as they sometimes have not, and as I have very frequently seen them almost in contact with the limb, that is, not  $\frac{1}{4}$  a second of time distant in passing a wire; I think they can scarcely be in such a hollow, below his surface, as the Doctor describes. To me indeed, by the brighter light often adjoining to them when near the limb, they have rather put on the appearance as if they were in the crater of a volcano on the top of an eminence, which then turned its side towards us; and if so, the spot would appear somewhat nearer to the limb than it actually was. I have indeed never seen any protuberance on either limb of the sun, as I have on the moon; but I have often observed, near the eastern limb, a bright facula just come on, which has the next day shown itself as a spot; though I do not recollect to have seen such a facula near the western one, after a spot's disappearance. Yet I believe both these circumstances have been observed by others, and perhaps not only near the limbs.

As to the *nebulæ*, they are certainly not always, though they usually are, quite round each spot, or each cluster of spots; neither are they always externally convex. Nothing therefore can be concluded from that circumstance. Besides spots are sometimes quite without any *nebulæ* at all; or none that I could perceive with any power of my glass. What the spots, or their *nebulæ*, are, I pretend not to guess. To me they appear as if they were adjoining to the surface: though that is doubted by better astronomers, who have calculated their motions. The circumstance of the *faculæ* being sometimes converted into spots, I think I may be sure of. That there is generally, perhaps always, a mottled appearance over the face of the sun, when carefully attended to, I think I may be as certain. It is most visible towards the limbs; but I have undoubtedly seen it in the centre: yet I do not recollect to have observed this appearance, or indeed any spots, to-

wards his poles. Once I saw, with a 12-inch reflector, a spot burst to pieces while I was looking at it. I could not expect such an event; and therefore cannot be certain of the exact particulars; but the appearance, as it struck me at the time, was like that of a piece of ice when dashed on a frozen pond, which breaks to pieces and slides on the surface in various directions. I was then a very young astronomer; but think I may be sure of the fact. Perhaps I may be thought a young astronomer still, for throwing out these rough observations and crude thoughts: but whatever they be, if my errors shall lead others into inquiries which may be productive of certainty, their end will be answered.

*XXXV. An Account of a Woman accidentally Burnt to Death at Coventry.*  
By B. Wilmer, Surgeon, Coventry. p. 340.

Mary Clues, of Gosford-street in this city, aged 52 years, was of an indifferent character, and much addicted to drinking. Since the death of her husband, which happened about 1½ year before, her propensity to this vice increased to such a degree, that, as Mr. W. was informed by several of her neighbours, she had drunk the quantity of 4½ pints of rum, undiluted with any other liquor, in a day. This practice was so familiar to her, that scarcely a day had passed for a year before her death, but she swallowed from ½ a pint to a quart of rum or aniseed-water. Her health gradually declined; and, from being a jolly, well-looking woman, she became thinner, her complexion altered, and her skin became dry. About the beginning of Feb. 1774, she was attacked with the jaundice, and took to her bed. Though she was then so helpless, as hardly to be able to do any thing for herself, she continued her old custom of dram-drinking, and generally smoked a pipe every night. No one lived with her in the house. Her neighbours used, in the day, frequently to come in to see after her, and in the night, commonly, though not always, a person sat up with her.

Her bed-room was next the street, on the ground-floor, the walls of which were plastered, and the floor made of bricks. The chimney was small, and there was a grate in it, which, from its size, could contain but a very small quantity of fire. Her bedstead stood parallel to, and at the distance of about 3 feet from, the chimney. The bed's head was close to the wall. On the other side the bed, opposite the chimney, was a window opening to the street. One curtain only belonged to the bed, which was hung on the side next the window, to prevent the light being troublesome. She was accustomed to lie on her side, close to the edge of the bedstead, next the fire; and on Sunday morning, March the 1st, she tumbled on the floor, where her helpless state obliged her to lie some time, till a neighbour came accidentally to see her, who with some difficulty got her into bed. The same night, though she was advised to it, she refused to have any one to sit up with her; and at ½ past 11, one Brooks, who

was an occasional attendant, left her as well as usual, locked up her door, and went home. He had placed 2 bits of coal quite backward on the fire in the grate, and put a small rush-light in a candlestick, which was set in a chair, near the head of the bed; but not on the side where the curtain was. At half after 5 the next morning, a smoke was observed to come out of the window in the street; and, on breaking open the door, some flames were perceived in the room, which, with 5 or 6 buckets of water, were easily extinguished. Between the bed and fire-place lay the remains of Mrs. Clues. The legs and one thigh were untouched. Except these parts, there were not the least remains of any skin, muscles, or viscera. The bones of the skull, thorax, spine, and the upper extremities, were completely calcined, and covered with a whitish efflorescence. The skull lay near the head of the bed, the legs toward the bottom, and the spine in a curved direction, so that she appeared to have been burnt on her right side, with her back next the grate. The right femur was separated from the acetabulum of the ischium; the left was also separated, and broken off about 3 inches below the great trochanter. The connection of the sacrum with the ossa innominata, and the inferior vertebræ of the loins were destroyed. The intervening ligaments kept the vertebræ of the loins, back, and neck together, and the skull was still resting on the atlas. When the flames were extinguished, it appeared that very little damage had been done to the furniture of the room; and that the side of the bed next the fire had suffered most. The bedstead was superficially burnt; but the feather bed, sheets, blankets, &c. were not destroyed. The curtain on the other side the bed was untouched, and a deal door, near the bed, not in the least injured. Mr. W. was in the room about 2 hours after the mischief was discovered. He observed, that the walls and every thing in the room were coloured black: there was a very disagreeable vapour; but he did not observe that any thing was much burnt, except Mrs. Clues; whose remains he saw in the state above described. He took away one of the bones (the remains of the sacrum) which he inclosed with this letter. The only way that he could account for it is, by supposing that she again tumbled out of bed on Monday morning, and that her shift was set fire to, either by the candle from the chair, or a coal falling from the grate. That her solids and fluids were rendered inflammable, by the immense quantity of spirituous liquors she had drunk; and that when she was set fire to, she was probably soon reduced to ashes, for the room suffered very little.\*

\* There are many other instances on record, besides the above, of the combustibility of the human body, in subjects advanced in years and addicted to the use of spirituous liquors. See a paper containing a collection of histories of this kind translated from the *Journal de Physique* in the 6th vol. of the *Phil. Magazine*.



*XXXVI. Experiments on Animal Fluids in the Exhausted Receiver. By D. Darwin, M. D., of Litchfield. p. 344.*

The ancient opinion, that air exists in some of the blood vessels, was exploded by the discovery of the circulation. But many of our modern theorists seem to have conceived, that an elastic vapour of some kind exists in the blood vessels, as they have ascribed the lunar and equinoctial diseases to the variations of atmospheric pressure. This opinion seems to have arisen from observing, that the skin rises, and that the vessels are distended, even to bursting, under a cupping-glass; when the pressure of the atmosphere is taken off from one part, and continues to act on all the remaining surface of the body: and would indeed, at first sight, appear to be demonstrated by the following experiments. About 4 oz. of blood were taken from the arm of one of the attendants, and immediately put under the receiver of an air-pump; and, as the air was exhausting, the blood began to swell, and to rise in bubbles, till it occupied above 10 times its original space.

As false reasoning is, in no science, of more dangerous consequence than in that of medicine, Dr. D. persuaded himself that the removal of this error might be thought worthy the attention of the R. S. In April 1772, Mr. Young, an ingenious surgeon at Shiffnal in Shropshire, and Mr. Waltire, an accurate lecturer in natural philosophy, made, at his request, the following experiments. 1. A part of the jugular vein of a sheep, with the blood in it, was included between 2 strict ligatures, during the animal's being alive, and being cut out with the ligatures, was immediately put into a glass of warm water, and placed in the receiver of an air-pump, it sunk to the bottom of the water, and would not rise when the air was diligently exhausted. It was then wiped dry, and laid on the brass floor of the receiver, and the air again exhausted, but there was not the least visible expansion of the vein, or its contents. 2. A ligature was put round the neck of the gall-bladder of the same animal, as soon as it was slaughtered; the gall-bladder, with the bile in it, was first put into water, in which it sunk, and was placed in the exhausted receiver of the air-pump; and was afterwards wiped dry, and laid on the brass plate at its bottom, as in the former experiment; but in neither case, on the greatest degree of exhaustion, did it show the least alteration of its bulk. 3. The neck of the urinary bladder of the same animal was well secured with a ligature, and contained about 2 or 3 oz. of fluid. The bladder sunk immediately on being put into warm water; but, on exhausting the receiver, many silver-like globules appeared on its surface; and it soon showed manifest signs of expansion, and rose to the top of the vessel. The same experiment was tried with it wiped dry, and laid on the floor of the receiver, and the result was, that its expansion and contraction were very perceptible to the eye.

In January 1773, by the assistance of Mr. Webster, an ingenious surgeon from Montrose, the above experiments were repeated in the following manner. A part of the vena cava inferior of a large swine, which was killed by some strokes on his head with an axe, was intercepted, when full of blood, between 2 ligatures. The part was about  $1\frac{1}{4}$  inch long, and held, by conjecture, near an ounce of blood; this was immersed in warm water as soon as it was cut out of the warm body, and immediately put into the receiver of an air-pump. The air was well exhausted, and again let into the receiver repeatedly, without any appearance of enlargement of the vein; which must have been easily perceivable by its ascending in the warm water. The same experiment was tried on the urinary bladder, with the same success, the urethra being tied with a ligature, while it was still in the body. The gall-bladder rose in the warm water, though the bile duct was tied before it was taken out of the body, and had air bubbles appearing on its sides, like globules of quicksilver, as happened to the urinary bladder in the experiments at Shiffnall; which, in both cases, was ascribed to some portion of cellular membrane adhering to the bladders, into the cells of which, at the time of cutting them out, some air had insinuated itself.

In these experiments the water, in which the animal parts were immersed, was warmed to about 100 degrees of Fahrenheit's scale, lest a greater degree of heat in the water might have raised an elastic vapour from these fluids, which did not naturally exist in the living animal; and all the parts were well cleared from the cellular membrane and fat; as it was imagined the atmospheric air might intrude itself into the cellular membrane, as is seen in tearing off the skin of animals recently killed, and which did indeed disappoint 2 of the above experiments, as was manifest from the silvery globules, which appeared on the surfaces of the bladders.

From the facts established by these experiments, Dr. D. thinks the following conclusions may be drawn. 1. That so great a change is produced in the blood; by its receiving, in its passage from the arm of the patient to the basin, a great admixture of atmospheric air, that the experiments afterwards made on its sensible or chemical properties are rendered very uncertain and erroneous; since the florid colour of the blood, its property of coagulation, and perhaps of putrefaction, may depend on this adscititious admixture of atmospheric air, and, at the same time, we see why so much less froth is produced in the operation of cupping, than from blood placed in the exhausted receiver of an air-pump; though perhaps as great a degree of vacuum is made in one case as in the other.

2. It is probable, from these facts, that animal bodies can bear much greater variations of the pressure of the atmosphere, than the natural ones, without any degree of inconvenience. Some who have ascended high mountains are said to have been seized with a spitting of blood; but as this never happens to animals

that are put into the exhausted receiver of an air-pump, where the diminution of pressure is many times greater than on the summit of the highest mountains, it is probable it was an accidental disease, or was owing to some violent exertions in ascending. And in the curious account Dr. Halley gives of his descending in a diving bell so low, as to have the weight of many atmospheres over him, no other complaint is recorded, but a disagreeable sensation, as he was descending, like something bursting in his ears, and which recurred at about the same depth of water in his ascent.

From the above observations of Dr. Halley on the sensation in his ears, when he descended and ascended in the diving bell, Dr. D. was led to imagine, that the air contained behind the tympanum in the vestibulum cochlea, and semicircular canals of the ear, had found or made itself a way into the Eustachian tubes, or into the external ear, by some undiscovered passage; and concluded, that a similar operation might be of service to some deaf people, where the immediate cause of their deafness might be owing to the excess or defect of this internal air. For this purpose a cupping glass, which had a syringe to exhaust it, was put over the ears of three different people, who were very hard of hearing. The inequality of the mammoid process of the temporal bone, made it necessary to put 2 or 3 circles of wash leather dipped in oil around the helix of the ear. On working the air-syringe, the external ear swelled; and became red; and at length the patients complained of pain in the internal ear, and the air was readmitted. One of these 3 patients heard considerably better immediately after the operation, and received permanent advantage; the others received neither benefit nor disservice. If this small degree of success from the use of the cupping glass, as so little pain or trouble attends the operation, should encourage other deaf persons to make use of it, it may be a means to give some light into the intricate diseases of this organ, the structure of the parts of which, and their uses are yet so little understood.

*XXXVII. An Account of a Storm of Lightning observed March 1, 1774, near Wakefield, in Yorkshire, by Mr. Nicholson, Teacher of Mathematics in Wakefield. p. 350.*

On the 1st of March, about half past 6 in the evening, as I was returning from Crofton, a village near Wakefield, I saw, says Mr. N., in the north-west, a storm approaching; the wind, which had been strong all the day, setting from the same quarter; and as in the afternoon of the same day, there had been some violent showers of hail, I made the best of my way to the turnpike at Agbridege. The air was so much darkened, before the storm began, that it was with difficulty I found my way. When I was about 300 yards from the turnpike, the storm began; when I was agreeably surprised with observing a flame of light,

dancing on each ear of the horse that I rode, and several others much brighter on the end of my stick, which was armed with a ferule of brass, but notched with using. These appearances continued till I reached the turnpike-house, where I took shelter. Presently after, there came up 5 or 6 graziers, whom I had passed on the road. They had all seen the appearance, and were much astonished. One of them, in particular, called for a candle, to examine his horse's head, saying, "It had been all on fire, and must certainly be singed."

After having continued about 20 minutes, the storm abated, and the clouds divided, leaving the northern region very clear; except that, about 10 degrees high, there was a thick cloud, which seemed to throw out large and exceedingly beautiful streams of light, resembling an aurora borealis, towards another cloud that was passing over it; and, every now and then, there appeared to fall to it such meteors as are called falling stars. These appearances continued till I came to Wakefield; but no thunder was heard. About 9 o'clock a large ball of fire passed under the zenith, towards the s. e. part of the horizon. I have been informed that a light was observed on the weathercock of Wakefield spire, which is about 240 feet high, all the time that the storm continued.

*XXXVIII. Account of a Woman enjoying the Use of her right Arm after the Head of the Os Humeri was cut away. By James Bent, Surgeon, at Newcastle. p. 353.*

Mr. White, of Manchester, in the history of an operation performed on the humerus, published in his treatise, entitled, *Surgical Cases, with Remarks*, and read before the R. S., Feb. 9, 1769; asserts, that he sawed off the upper head of that bone; and that his patient enjoyed the entire use of the joint. As the supposition of the head of the bone, with its ligaments, &c. being regenerated, must appear a little marvellous, and may prevent some from paying that attention to the operation, that it certainly merits, Mr. B. flattered himself the following case would not be unacceptable to the R. S., as it proves, that the operation is not only practicable, but adviseable; and, at the same time, points out the nature of Mr. White's mistake. In pl. 6, fig. 1, he has given a drawing of the bone he cut off; the bare inspection of which is sufficient to convince any one, that it could be only the body of the humerus that was carious, and separated from its epyphysis; as the round head, with its cartilage, is wanting; and Mr. B. believes, there are few instances where the whole head of any bone is so entirely destroyed, in 2 or 3 weeks, by a caries, as that drawing represents. Hence it appears that the joint, with its capsular ligament, remained in a sound state. He is further confirmed in this opinion, by attending to the description Mr. W. has given of his mode of performing the operation, (vide p. 58) where he says, "that he began his incisions at the orifice which was

situated just below the processus acromion." Now as the processus acromion reaches a little over the joint, his beginning his incision below that must, of course, be below the insertion of the capsular ligament.

Mary Turner, a farmer's daughter, of Ipstones, in this county, applied to Mr. B. in October, 1771, on account of an abscess in the joint of her right shoulder, with which she had been afflicted near 3 years. On examining it, he found 3 apertures; 2 near the middle and lower edge of the clavicle; and the 3d, near the insertion of the pectoral muscle into the humerus. By introducing 2 probes, from the upper and lower orifices, they easily met in the joint, the opening into which, through the ligament, seemed to be very small, and he could perceive the head of the humerus carious. As in this case, there seemed nothing to be proposed for her relief, but either to amputate the arm, or by an opening, to cut away the head of the bone. He determined on the latter; and accordingly began an incision from the upper orifice, near the clavicle, and continued it over the joint to the insertions of the pectoral muscle: but finding a single incision too small, to allow him to get at the head of the bone readily, he separated a part of the deltoid muscle from its insertion into the clavicle; and likewise a little of its insertion into the humerus; which gave him liberty to come at the joint, the capsular ligament of which, from frequent inflammation, was so thickened, and kept the head of the bone so close to its socket, that it was with difficulty he could introduce a spatula between them. This likewise, after opening the ligament, prevented the head of the bone from rising out of its socket, on pressing the elbow backward, as is common in performing the operation on a dead body, when the joint is in a sound state; so that he was obliged to separate it quite round, before he was able to come at the bone with the saw. He then moved the elbow backwards, and brought the head of the bone over the pectoral muscle, as he found it impossible to saw it directly across, as Mr. White directs; without leaving a considerable portion behind, that had been laid bare with the knife, and which, in all probability, must have exfoliated. By placing a card between the edge of the deltoid muscle and the bone, and the saw within the incision, with its point into the joint, he cut off all that had been deprived of the periosteum, and had no exfoliation; nor had he occasion to take up one artery. As the tendon of the biceps muscle was cut through, he kept the fore-arm suspended. The patient walked from his house to her own lodgings; her pain was not very considerable, and she recovered, by the common treatment, without any bad symptom. She left this town in 6 weeks after the operation.

By using her arm too freely when she got home, the cicatrix was torn open about 1½ inch, which retarded its healing for 3 weeks longer; but from that time she had remained well. She had the perfect use of the fore-arm; could,

raise her elbow about 5 or 6 inches from her side, put her arm back, lace her stays, put on her cap, sew, and do any business, as well as ever, that does not require the elbow to be more raised. The upper end of the humerus played about an inch below the point of the scapula; and the processus acromion and coracoides appeared on each side of the cicatrix, at nearly equal distance. He mentioned this only to point out more exactly the course of the incision.

*XXXIX. Continuation of an Experimental Inquiry concerning the Nature of the Mineral Elastic Spirit, or Air, contained in the Pouhon Water, and other Acidulæ. By W. Brownrigg, M. D., F. R. S. p. 357.*

PROP. 1. *The ferruginous and absorbent earths, contained in the Pouhon water, are kept dissolved in it, by means of the mephitic air\* to which those earths are united.*—In an inquiry concerning the nature of the mineral and elastic spirit, or air, contained in this water, published in the Trans. of the R. S., vol. lv. [Abrid. vol. xii, p. 235] it has been shown, that when the Pouhon water is excluded from all contact with the common air, in such manner that the mephitic air which it contains has free liberty to fly from it into an empty bladder, this air does not separate from the water by any spontaneous motion, as it would from its rare texture and elastic force, were it at liberty to exert these its qualities: but, on the contrary, in this situation, it remains united to the other ingredients of the water, when exposed to the most intense heat that we usually observe, in the open air, in this our climate. It has been further shown, in the 2d experiment, that this elastic fluid, when excluded from common air, in the manner before related, is but slowly expelled from the Pouhon water by a heat of 110 degrees of Fahrenheit's thermometer, though such heat is sufficient to raise water, a much heavier body, in distillation: and so closely is air united to the other ingredients of the water, that it is not wholly expelled from them by a scalding heat of 160 or 170 degrees of the scale, when exposed to it for 2 hours.

Which experiments therefore prove, that this air is not detained in the Pouhon water by the pressure of the atmosphere, or by any other external force, as is the air with which beer, or other fermenting liquors, are often surcharged, while they are confined in bottles; but that this elastic fluid is equally mixed with the watery element, and with the other ingredients of which this mineral water is composed, and exists with them in a state of solution, or in a fixed state, being attached to the water, and to the other ingredients dissolved in it, by a force sufficient to keep them all united together in one uniform compound, while this force is not removed by some external cause.

It further appears, from the same experiments, that so long as this air continues united to the other ingredients of the Pouhon water, its martial and

\* Now termed Carbonic Acid Gas.

absorbent earths do also remain suspended in it; but, so soon as any part of this air is expelled by heat, those earths begin to separate from the water, which then becomes white and turbid; and when, by continuance of the heat, more of this air is expelled, more of the earthy particles also separate from the water, in the same proportion as its air is separated from it; and while only a small portion of the air remains, some portion of the martial earth also remains dissolved in the water, as appears from its giving a slight tinge of the purple, when mixed with galls: but none of these earths are any longer detained in the water, than while it continues impregnated with some mephitic air: when this air is entirely separated from the water, it is wholly decomposed, having lost its distinguishing brisk and pungent taste, and its power of striking a purple colour with galls; its more volatile and elastic principles being exhaled, its metalline and absorbent earths then subside in a white flocculent sediment, and no other substance remains dissolved in the water, save only the small portion of alkaline and neutral salts, which enter its composition.

From this short recapitulation of the abovementioned experiments, it therefore appears, that the Pouhon water undergoes a decomposition when its air is expelled from it by means of heat. The opposite extreme of cold is also found to produce the same effect of decomposing the Pouhon water, when this its aerial principle is expelled from it by means of congelation. For having poured some of this water into open tin vessels, that were placed in the common freezing mixture of sea salt and snow, as soon as the water began to shoot into ice, at the bottom and sides of the vessels, very minute bubbles of air incessantly arose in it, and were discharged from its surface with such force, as to carry with them small particles of the water to a considerable height; and continued thus to fly off, till all the water was congealed. The ice was very white, from the minute bubbles of air, which were every where interspersed through it, and by which the frozen water considerably increased in bulk, so as to rise at its surface into a very convex form. The water, when thawed, was white and turbid, and soon let fall its metallic and absorbent earths in a white sediment: it then had almost lost all its taste; and, being mixed with tincture of galls, only gave a slight purple tinge. By a 2d congelation, it seemed almost entirely deprived of its air, and, with it, of the remaining part of its white earths; and, when decanted from its sediment, no longer struck a colour with galls. From these experiments it therefore appears, that as soon as this water is deprived of its air, whether it be by heat or by cold, it is no longer capable of keeping those earths dissolved, which, while it is impregnated with this air, continue suspended in it.

In these decompositions of the Pouhon water, by heat and by cold, no volatile spirit, either acid or sulphureous, nor any other subtile matter, has been found to fly from it, save only its mephitic air: while this air is present in the

water, its martial and absorbent earths remain dissolved in it; as soon as this air is separated from the water, in whole or in part, those earths, either in the whole or in part, do also separate from it, and are no longer suspended in it, than while they are united to a due proportion of this aerial solvent. Whence it appears, that this mephitic air is the medium by which the metalline and absorbent earths, contained in the Pouhon water, are held in solution; and, contrarywise, that those earths are the medium, by which this air is more firmly united to the watery element in this compound, in which it enters as a principal ingredient, and, by its solution in the water, and its union with these earthy substances, from a very rare volatile and elastic body, is reduced to a fixed state.

This dissolving power of mephitic air may further be proved from the recombination of the Pouhon water, by adding to it the air expelled from it by coction. But as Mr. Cavendish has already shown, that the absorbent earths of Rathbone Place water may be redissolved by the mephitic, or fixed air, which had been extracted from that water; and as Mr. Lane has also demonstrated, that iron is rendered soluble in water, by the medium of mephitic air, Dr. B. did not think it necessary at that time to give an account of his experiments on the same subject; but as those experiments contain some phenomena that have not yet been noticed, he may perhaps offer them to the public on some future occasion.

*Schol. 1.*—From the foregoing experiments, it appears that the mephitic air and martial earth, contained in the Pouhon waters, strongly attract each other, and, uniting together, form a concrete soluble in water, and readily distinguished in it, by the peculiarly brisk acidulous taste, which it receives from this aerial principle, joined to a rough subastringent taste, which proceeds from the iron. This concrete, like other vitriols of iron, strikes a black colour with galls, and may well be esteemed a saline body of the neutral kind, of which the mephitic air constitutes the spirituous solvent, and the martial earth its base. It further appears, that the mephitic air is possessed of all the properties, by which some of the chemists have distinguished those pure and simple bodies, or spirits, which by them are esteemed, in their own nature, and of themselves, saline, and which, in union with other bodies, form salts that are more compound. For this aerial solvent, in like manner with the pure acid spirits, is soluble in water, and imparts to it its peculiar sharp and acidulous savour: also, in combination with various metalline and absorbent earths, this volatile elastic spirit, like those acids, forms various saline concretes of the neutral kind; inasmuch as those metalline and absorbent earths, when united to this elastic spirit, are thus rendered soluble in water; and, in union with it, acquire peculiar savours, resulting in part from this their spirituous principle, and in part also from the particular kind of earth with which it is combined. This air therefore, considered in the relation which it bears to several earthy substances, and to water, considered also as it impresses



the organs of taste, with its peculiar brisk and acidulous savour, may justly be stiled a mineral elastic spirit of a saline nature, and is sufficiently distinguished from all other saline spirits, by its great rarity, and by its ærial nature. How far, and under what laws, this relation between mephitic air and various saline earths, and other bodies, may be extended, has not yet been fully discovered: suffice it in this place to remark, that a class of saline bodies of a neutral nature are here detected, composed of various earthy bases, united to a volatile ærial spirit, all of which agree in one common solvent, the mephitic air, but differ from each other, according to the nature of the base to which this air is united.

The agreement of these saline concretes with neutral salts in these essential properties, by which these last are distinguished from other more simple saline bodies, will further appear from their decomposition; which is effected by those various ways, and under the same laws, by which all other neutral salts are decomposed; namely, by all those different ways, by which the acid spirits, and the terrene or alkaline bases of neutral salts, can be separated from each other.

For, first, the ærial spirit of these saline concretes, is forced, by fire, from its union with the earthy base, which it holds dissolved in water, in like manner as the acid spirit of other neutral salts are expelled by fire from the more fixed principles, which enter the composition of those salts. The degree of heat required to separate the acid spirit of neutral salts, from their more fixed alkaline or earthy base, varies in the decomposition of almost every different kind of salts; and the extreme volatility and expansive force of this æreo-saline principle renders it more easily separable, by heat, from the fixed principles to which it unites, than any other kind of saline spirit.

Secondly. The saline concretes, formed with this ærial solvent (in like manner as other neutral salts), are decomposed by the addition of stronger acids, which more powerfully attract the terrene or metallic base of these concretes, than it is attracted by their light and subtle ærial spirit, and detaches from them the ærial solvent to which those earths were before united. All acids, found in a liquid form, have this effect from the light vinous acids to the most ponderous acid of vitriol; so that the affinity between these metalline and absorbent earths, and this their ærial solvent, is less than that which exists between the same earths and all the known acid spirits. In all additions of these acids to the spirituous or acidulous waters, an effervescence has been observed, not readily accounted for, by those who suppose an acid to predominate in those waters. The conflict and discharge of air here arises from the expulsion of the ærial principle from its terrene base; in like manner as the acids of sea salt and nitre are expelled, with effervescence, from their alkaline bases, by the more powerful

acid of vitriol. And here, by the way, it may be proper to remark, that the vitriolic acid, when mixed with the acidulæ and other chalybeate waters, does not preserve those waters from decay, as Hales, and others, after him, have supposed; but, on the contrary, destroys their texture, or decomposes them, by expelling their elastic spirit, and entering into new combinations with their earthy principles; thus forming a new compound, less perishable indeed than the former, but also less efficacious in the cure of many diseases. When Rhenish wine is added to the acidulæ, the large quantity of air that flies off may, in part, proceed from the wine; but when Dr. B. mixed the vitriolic acid with Pouhon water, a considerable quantity of air was indeed discharged; but not the whole which that water holds in solution. He therefore conjectured, that some part of the air, contained in that water, might be imbibed by the superabundant acid, which he used in the experiment, and that more mephitic air might perhaps have been expelled from the water, had he only mixed with it the exact quantity of this acid, that was required to dissolve the earthy substances contained in it.

Thirdly. These saline concretes, contained in the Pouhon water, and other acidulæ, are subject to decomposition, not only from acids, as before related, but also from alkalies, whether fixed or volatile: all which more powerfully attract this subtile ærial principle than it is attracted by the martial and absorbent earths, to which it is united in those waters. And here again appears an exact agreement between these æreo-saline concretes, and various neutral salts, in the mode of their decomposition. For the ammoniacal salts (which are all composed of the volatile alkali, united to an acid spirit, either muriatic, nitrous, or of some other kind) as soon as one of the fixed alkalies, or quicklime, is added to any of them, the acid spirit which it contains, quitting its union with the weaker volatile alkali, this last is let loose; and the stronger alkali, or quicklime, takes its place; between which and the acid spirit a new combination is formed. The same happens when any alkali, either fixed or volatile, is added to the acidulæ; their elastic spirit then quits the ferruginous and absorbent earths, to which it was joined, and forms a new combination with the alkali, by which it is more powerfully attracted than by these earthy substances. These earths therefore, being no longer suspended in the water by the ærial solvent, render it turbid and milky, until they have gradually subsided in it, in the form of a white sediment: for such is the native appearance of the martial earth, as well as of all the other earths contained in these waters, as will be shown hereafter. In these decompositions of acidulous waters, by means of alkalies, no effervescence, or discharge of air bubbles, takes place; for here the air is all absorbed by the alkali added, and not expelled from the water, as it is in the decomposition of the same waters, by means of stronger acids.

When the acidulæ are mixed with common soap, a two fold decomposition

takes place. The fixed alkali, quitting the unctuous substances, to which it was joined in the soap, unites itself to the ærial spirit, or mephitic air, of those waters, while this air, at the same time, deserts the earthy substances with which it was before combined. The same new combinations seem to take place, when soap is mixed with any of those waters which are usually called hard; many of which waters have been found to contain an earthy substance, dissolved by means of this subtile ærial principle.

The above observations and experiments show an exact agreement, in the several ways by which the various neutral salts; and those saline concretes, formed of mephitic air united to an earthy base, are decomposed. It ought however here to be remarked, that the saline concretes, which exist in the Pouhon water, in a dissolved state, though evidently of the neutral kind, have not hitherto been obtained in a solid form; owing perhaps, in some measure, to the great volatility of their spirituous principle; but chiefly to their being subject to decomposition, from the precipitation of their earthy base, by means of common air, during the evaporation of the water in which they are dissolved, as will be shown hereafter.

The mephitic air of the acidulæ, though it is soluble in water, and imparts to it its brisk and pungent taste, which has been usually stiled subacid; and though it produces effects exactly similar to those of acid spirits (by readily uniting to various earthy substances, which of themselves are not soluble in water, but, by their union with this ærial fluid, are rendered soluble in it, and communicate to the water peculiar savours, and form in it saline concretes of the neutral kind; which concretes, so formed, are again separable into their component ingredients, by all those ways by which the acid and alkaline principles of other neutral salts are separable from each other) yet it differs from all acid spirits, found in a liquid form, in its rare texture and in its elastic quality, and in not striking a red colour with syrup of violets, and other blue tinctures of vegetables; which change, in the blue colour of those tinctures, is usually esteemed a test of the presence of an acid. Besides the trials which have been made, by mixing syrup of violets with pure water, impregnated with various kinds of mephitic air, in which no change in the colour of the syrup was observed; he had for several days suspended pieces of linen, that had been dyed blue with fresh juice of violets, in the mephitic air of spa water, and also in that of chalk; and, when the linen was taken out of the said air, did not perceive its blue colour in any wise changed, though the same pieces of dyed linen were instantly turned of a green colour, when exposed to the fumes of spirit of hartshorn. Whether therefore, and under what relations, this æreo-saline spirit may merit the title of an acid, he leaves to the determination of others. Such however it has appeared to be to many philosophers, since this mephitic air is doubtless the same with the acidum vagum fodinarum of Boerhaave and others; and with the

acidum centrale perpetuum inexhaustibile of Beccher; with the spiritus sulphureus aëreo-æthereo-elasticus of Hoffman; and the sal embrionatus and sal esurinus of the sagacious Helmont, which, he says, corrodes the ore of iron, and with it forms a volatile vitriol in the Pouhon water. All these, and many other philosophers, had acquired some knowledge of this subtile aëreo-saline principle from contemplating its effects; but, not having obtained it in a palpable form, were unacquainted with several of its principal properties.

From considering the great subtilty of this aëreo-saline principle, its power of dissolving many earthy substances, with its property of uniting readily to water, and with it, of pervading the very minute vessels of the animal frame, without injuring them, as stronger acids do by their corrosive quality, we may thence form some judgment of the great efficacy of this air, as a de-obstruent and solvent, in many diseases of the human body, arising from preternatural concretions and obstructions thence ensuing. If to these we add the great antiseptic powers of this kind of air, which it possesses in common with acids, and which were first detected by Sir John Pringle, and have since been more fully explained by Mr. Macbride and Dr. Priestley; we then, in some measure, may account for those extraordinary effects which this kind of air is found to produce, in the cure of many obstinate diseases, with which mankind are afflicted.

*XL. Particulars of the Country of Labradore, extracted from the Papers of Lieut. Roger Curtis, of H. M. Sloop the Otter. p. 372.*

This vast tract of land is extremely barren, and quite incapable of cultivation. The surface is every where uneven, and covered with large stones, some of which are of amazing dimensions. There are few springs; yet throughout the country there are vast chains of lakes or ponds, which are produced by the rains, and the melting of the snow. These ponds abound in trout, but they are very small. There is no such thing as level land. It is a country formed of frightful mountains, and unfruitful vallies. The mountains are almost devoid of every sort of herbage. A blighted shrub, and a little moss, is sometimes to be seen on them; but in general the bare rock is all you behold. The vallies are full of crooked low trees, such as the different pines, spruce, birch, and a species of the cedar. Up some of the deep bays, and not far from the water, it is said however there are a few sticks of no inconsiderable size. In short, the whole country is nothing but a vast heap of barren rocks.

The climate is extremely rigorous. There is but little appearance of summer before the middle of July; and in September the approach of winter is very evident. All along the coast there are many rivers, which empty themselves into the sea; yet there are but few of any consideration, and you must not imagine that the largest are any thing like what is generally understood by a river. Cus-

tom has taught us to give them this appellation, but the most of them are nothing more than broad brooks, or rivulets. As they are only drains from the ponds, in dry weather they are every where fordable; for running on a solid rock, they become broad, without having a bed any depth below the surface of the banks.

There is no variety of animals in this rocky country, nor are they at all numerous. Here are the rein-deer; the females have horns, which nature has given them to procure food, for with these they beat away the snow in winter, and by that means come at the tops of trees, which, during the inclemency of that season, is their only sustenance. There are bears black and white, wolves, the carkashew, foxes, porcupines a great many, the mountain-cat, martins, beavers, otters, hares, and a few ermine. A venomous reptile or insect, is not to be found here, except toads, and these are extremely rare. The whole country is filled with very small flies, which are exceedingly tormenting. Here are eagles, hawks, the horn-owl, and the red-game, with a smaller sort which resemble them, called the spruce-partridge: these we may call the constant inhabitants of the feathered kind. Of sea-birds, there are a great variety.

In the summer the woods are visited with many sorts of little birds, and some of them of beautiful plumage. They breed here, but towards winter they seek a happier climate. In the autumn there come a prodigious quantity of curlews. They are about the size of a woodcock, shaped like them, and nearly of the same colour; extremely fat, and most delicious eating. They continue here but a very little while, nor is it known whence they come, or whither they go. The principal fish are whales, the cod-fish, and salmon. Of shell-fish, there are but few sorts, and these in no great plenty. Lobsters there are none at all; which is very remarkable, for at a particular part in the Straits of Bellisle, not more than 5 or 6 leagues from Newfoundland, there are great abundances.

It is not surprizing that such a country as this should be thinly inhabited. The human species upon this extensive territory are but few; and such as we know of are extremely savage. The people of this country form various nations or tribes; and are at perpetual war with each other. Formerly the Esquimaux, who may be called a maritime nation, were settled at different places on the sea-coast, quite down to the river St. John's; but for many years past, whether it has been owing to their quarrels with the mountaineers, or the encroachments of the Europeans, they have taken up their residence far to the north. A good way up the country are people distinguished by the appellation of mountaineers, between whom and the Esquimaux there subsists an unconquerable aversion. Next to the mountaineers, and still farther westward, is a nation called the Escopics: and beyond them, are the Hudson Bay Indians, with whom the world is but little acquainted. There are doubtless, in such a vast tract of land, a

great number of other nations, but of whom we have not the least information.

The mountaineers are esteemed an industrious tribe; and for many years had been known to the French traders. Their chief employment is to catch fur, and procure the necessaries of life. They are extremely illiterate, but generally good-natured; and are reckoned to be less ferocious than any other of the Indians. They come every year to trade with the Canadian merchants, who have seal fisheries on the southern part of the coast, and have the character of just dealers. They are immoderately fond of spirits; for which, with blanketing, fire-arms, (in the use of which they are remarkably dexterous), and ammunition, they truck the greatest part of their furs. Their canoes are covered with the rind of birch; and though so light as to be easily carried, yet sufficiently large to contain a whole family and their traffic. By means of the multitude of large ponds throughout this country, they convey themselves a vast distance in a very little time. Whenever they find a pond in their way, they embark on it, and travel by water; when its course alters, and by following it they would lengthen their distance any thing considerable, they land, place their canoe on their head, and carry their baggage on their shoulders, till other water gives them an opportunity of reembarking. They are most excellent travellers. They bear inconceivable fatigue with astonishing patience, and will travel 2 days successively without taking any sort of nourishment. These Indians are of a deeper colour than the Esquimaux; and are low of stature. Though of a robust constitution, their limbs are small, and extremely well adapted to the rocky country they are continually traversing. They have no hair, except on the head. For many years they have dressed their food, which they boil to a jelly; whereas the other Indians eat every thing raw. It is their custom to destroy the aged and decrepid, when they become useless to the society, and burthensome to themselves. They have been questioned on this seeming inhumanity; and perhaps their reasons are not totally devoid of sound philosophy. They tell you, that as it is with difficulty they procure the necessaries of life, they can admit of none who do not contribute towards acquiring it; that having no fixed residence, and it being impossible to carry the helpless with them, as they are obliged to be continually traversing the country; they ask, if it is not better to put an end to miserable beings, than suffer them to perish with cold and hunger? The son generally does this kind office for the father; and, it having ever been a practice among them, they wonder at our considering it as an act of inhumanity.

The Esquimaux Indians, inhabiting the sea-coast of the northern part of Labradore, are doubtless from Greenland. They are a very deep tawny, or rather of a pale copper-coloured complexion. They are inferior in size to the generality of Europeans; and but a few among them are of good stature. They

bear a very near resemblance to the Laplanders, both in their persons and customs. They have beards, so have the Greenlanders, and indeed so have the inhabitants of Lapland; whereas the Iroquois, the Hurons, the Escopics, and the Mountaineers their neighbours, have hair no where except on the head. These Indians, in general, are not very disagreeably featured, though some among them are extremely ugly. They are flat-visaged, and have short noses. Their hair is black and extremely coarse. Their hands and feet are remarkably small. The women load their heads with large strings of beads, which they fasten to the hair above the ears; and they are fond of a hoop of bright brass, which they wear as a coronet. Their dress is entirely of skins, except those who have trafficked for a little blanketing. It consists of a sort of hooded close shirt, breeches, stockings, and boots. They wear the hairy side towards them, according to the seasons; and between the dress of the different sexes there is no variety, except that the women wear monstrous large boots, and their upper garment is ornamented with a tail. In the boots they occasionally place their children; but the youngest is always carried at their back, in the hood of their jacket. They have no sort of bread; but live chiefly on the flesh of seal, deer, fish, and birds. Till very lately they ate every thing raw, and putrefaction was deemed no objection.

In the winter they live in houses, or rather caverns, for they are sunk in the earth. In the summer they dwell in tents, which are made circular with poles, and covered with skins sewed together. The house consists of one room, and though not very large, yet it contains several brothers or other relations, with their wives and children. Their tents are still more crowded; because, as the whole summer they are generally rambling up and down the coast, they endeavour to diminish their baggage as much as possible. They are without any government; and no man is superior to another, but as he excels in strength or in courage, and in having the greatest number of wives and children. Being entirely without laws, general censure is the only punishment for the most detestable crimes. They have no marriage ceremony. A wife is considered as property, and a husband lends one of his wives to a friend. The wives are given very early in marriage, frequently several years before consummation; and the reason of this is, because the girl's father, by that means, has one less in family to provide for.

The Esquimeaux men are extremely indolent; and the women are the greatest drudges upon the face of the earth. They do every thing except procure food, and even in that they are frequently assistants; so that they are at continual labour. They sew with the sinews of deer, and their needle-work is amazingly neat. Their language is the same as the Greenlanders. It is not altogether devoid of harmony, and the women have very delicate voices. These Indians

are strangers to jealousy; they do not appear to be at all quarrelsome, and they very seldom steal from one another. They do not seem very passionate; but woe be to the woman that offends her husband. If polygamy was not allowed among them, their numbers would be very few. Some of the women bear many children; but, in general, they are by no means fruitful. The wives live happily together; and, if deserving, share equally in their husband's favours. These Indians cannot reckon numerically beyond six; and their compound numbers reach no farther than twenty-one. Every thing beyond is a multitude. They navigate their shallops without a compass in the thickest fogs, and are very good coasters. They have always a vast number of dogs in their camp, which are of several uses. These animals serve as a guard; they are food; their skins are valuable for cloathing; and they draw their sledges in winter. They have not the power of barking, but their howl is hideous; they are large, and have a head like a fox, whereas the dogs of the Mountaineers are extremely small. The Samojedes and the Laplanders train the rein-deer to their sledges. The country of Labradore produces these animals; but they are only serviceable to the Esquimeaux for food and raiment. The weapons of these Indians are, the dart and the bow and arrow. They are not very expert in the use of either; though it is with these they defend themselves, and procure the necessaries of life.

As to their population, the Esquimeaux inhabitants of Labradore are far from being numerous, but little exceeding 1600; and those savages who inhabit the inland parts are still less numerous.

*XLI. An Account of some New Experiments in Electricity; containing, 1. An Inquiry whether Vapour be a Conductor of Electricity.. 2. Some Experiments to ascertain the Direction of the Electric Matter in the Discharge of the Leyden Bottle: with a New Analysis of the Leyden Bottle. 3. Experiments on the Lateral Explosion, in the Discharge of the Leyden Bottle. 4. The Description and Use of a New Prime Conductor. 5. Miscellaneous Experiments, made principally in the years 1771 and 1772. 6. Experiments and Observations on the Electricity of Fogs, &c. in pursuance of those made by Thos. Ronayne, Esq. By William Henley, F.R.S. p. 389.*

§ 1. *An Inquiry whether Vapour be a Conductor of Electricity.*

*Exper. 1.* I insulated a glass funnel (pl. 11, fig. 1,) into which the streams, from a capillary tube, were directed by the electricity. From this funnel; the electrified drops were received into a large insulated earthen dish; across which lay a long wire; and from its end hung a pair of light cork balls. On working the machine, after about 90 or 100 turns of the winch, and when fifty or sixty drops had fallen into the dish, the balls separated, and presently diverged, to the



distance of half an inch. Then taking off the electricity from all the bodies concerned, I blew the column of water out of the capillary tube, replaced it in the bucket, pointing towards the funnel as before, and worked the machine again, to try whether the electricity, issuing from the syphon, and passing through the air, might not electrify all the bodies, so as to separate the balls, without the jet of water; but no such event happened. I then replaced it, with the jet falling into the funnel as before; when it succeeded. I then tried it a second time, without the jet of water; and it failed. I thus repeated the experiment alternately, with, and without the jet, taking off the electricity of the apparatus carefully between the trials; till I was perfectly satisfied that the jet of water, received into the funnel, and thence falling into the insulated dish below, was the medium by which the balls, hanging from the end of the wire placed in it, became electrified. Hence I inferred, that vapour from boiling water, &c. must also be a conductor of electricity, though probably in a less degree, as being more dissipated. Having since repeated this experiment by receiving the electrified jet immediately into a large insulated dish, I observed the effect to be much greater.

*Exper. 2.* Having procured a tin vessel, somewhat resembling an eolipile, or chymical retort; I placed it over a small lamp, on my prime-conductor, (fig. 2,) and filled it about half full of boiling water. The nose of it was so situated, as to throw the electrified drops into an insulated dish, furnished with balls, as in the former experiments. After the water had been some time poured in, and I imagined enough had evaporated to have produced some drops in the neck; I examined the lip, to see whether any descended, but saw none. However, on giving the machine a turn or two, I was very agreeably surprized to see the electric streams issue exactly as from a capillary tube; and a few drops having fallen into the dish, the balls became electrical, and were attracted by my finger, at the distance of a half or three quarters of an inch. In a few turns more of the globe, they separated half an inch. I then threw out the water; and, clearing the vessel of its vapour, I remounted it on its stand, pointing towards the dish as before, to try whether the sharp edge on the lip of the vessel would not electrify the air sufficiently to separate the balls, as the evaporated water had done. I turned the winch a long time for this purpose; but the balls never diverged at all. I then poured in the boiling water a second time; and, when the drops began to fall, the 4th turn separated the balls; and the 10th caused them to diverge to the distance of half an inch; and in this state of repulsion they continued a considerable time after I had ceased to work the machine. I then took off the electricity with my finger, and again cleared the vessel of its water, &c. and having replaced it with the point as before, I worked the machine again as usual. The air was now become in some measure electrical; for, at the

7th or 8th turn the balls began to separate, and in 40 turns they were about  $\frac{3}{4}$  of an inch distant from each other. I then ceased to turn the winch any longer; but had no sooner stopped, than the balls began to close, and in a very few seconds they were in contact; whereas, in the former experiment, when the electrified drops were in the dish, on my ceasing to turn the globe, they showed no sign at all of converging; and I imagine would have remained separate a long time, if I had not taken off their electricity with my finger. I apprehend, therefore, from this experiment, that the vapour of hot water is a conductor of electricity.

*Exper. 3.* I hung on a string, as near to the ceiling of the room as I could, a pair of pith-balls, which, on working the machine a considerable time, diverged  $\frac{3}{4}$  of an inch, but no wider. Then sticking into the conductor a smoking deal match, and working the machine again, they presently separated to the distance of 2 inches. The match, when placed in the same situation, and not smoking, had no such effect.

*Exper. 4.* Having placed an earthen half-pint mug on a stand, properly insulated; I fixed to a large ball of brass, placed in the bottom of it, the end of a wire, 6 or 8 feet in length. The other end of the wire connected with the prime conductor of a small electrical machine, fig. 3. Over this mug, and as near to the ceiling of the room as might be, I suspended a pair of light cork balls. Then filling up the vessel with boiling water, I began to work the machine; and in 50 or 60 turns of the winch, observed the balls to separate  $\frac{1}{4}$ , or half an inch, from each other. I then took off the electricity of the bodies, emptied the vessel, and cleared it of the vapour; and having placed the apparatus in the same manner, I again worked the machine, for a longer time, but without effect. On replacing the boiling water, I succeeded as at first. At other times, when I have been able to separate the balls by the air alone, to a small distance, yet by pouring in the hot water, the vapour has presently increased their divergence from  $\frac{1}{4}$  or  $\frac{1}{8}$ , to half an inch distance, or in that proportion, according to the state of the atmosphere with respect to dryness or moisture. In short I have repeated these kinds of experiments so often, and many times with so much success, that there can be no doubt of vapour being a conductor of electricity.

*Exper. 5.* I insulated the rubber of the machine, and hung a pair of Mr. Canton's balls on the prime-conductor. On working the machine, and taking off a spark, or two, to draw off the electricity naturally inherent in the rubber, &c. I observed the divergence of the balls, which was very great, inso-much that the strings were bent: and on approaching the back of the rubber with a smoking green wax taper, just blown out, (the smoke of which was instantly attracted to it,) they diverged no wider. I then took off the balls, and

placed my own electrometer in its stand, on the prime-conductor, fig. 4; and having taken off a spark or two, as before; I again worked the machine, to observe the repellency of the index from the stem; and found it constantly to vibrate between 5 and 10 degrees of the quadrant, which was divided into 15. I then brought the smoking taper within 4 or 5 inches of the back of the rubber, as before; and observed, that on the attraction of the smoke to it, the index presently began to rise, and in a very short time got up to right angles. I repeated the experiment several times, with the same success. I then tried the experiment by bringing my finger to the same distance from the rubber, and pointing towards it; but this, in many trials, had not the least effect. The taper likewise, when held at the same distance, and not smoking, had no effect at all. I am convinced therefore, that the smoke was the medium which conveyed the electricity from my hand to the insulated rubber.

*Exper. 6.* I placed on a stand, on the prime-conductor, a piece of smoking wax taper, fig. 5, when immediately on working the machine, the smoke, from a large and diffused volume, was much contracted, and its motion upwards greatly accelerated. I then took off the electricity of the conductor, and held a pair of cork balls a quarter of an inch diameter, hung on threads  $2\frac{1}{4}$  inches long, perpendicularly over the rising smoke; and as high as I could possibly reach, standing on a chair; this might raise the balls about  $5\frac{1}{4}$  feet above the prime-conductor; when, working the machine, in a few seconds the balls separated to half an inch distance. I then removed the taper, but could not perceive that the balls were at all affected without it; but on replacing it, they separated as before. I repeated the experiment several times, with and without the taper, and the different effect was constantly as above recited. I then set a tin saucer on the stand, and placed on the saucer a half pint mug of boiling water, fig. 6; and over this water I presented the balls in the rising vapour; as had before been done in the smoke. On working the machine a few seconds, the balls diverged to the distance of the 12th part of an inch. On removing the water, and presenting the balls as before, they never separated at all, though I worked the machine for a longer time; but on replacing the water, in a few seconds the balls diverged as at first. These experiments I repeated several times, and always with the same success. The smoke therefore, in the first experiment, and the vapour of the hot water in this last, was certainly the medium which conveyed the electricity from the prime conductor to the balls: and I think I may now very safely pronounce, that smoke, and the vapour of hot water, are absolutely conductors of electricity; though smoke is a far better one than the vapour of hot water, and both of them are exceedingly bad ones.

§ 2. *Of the Direction of the Electric Matter, in the Discharge of the Leyden Bottle.*

*Exper. 1.* Light a small wax taper, and place it, with the flame exactly be-

tween 2 brass balls, A and B, about 2 inches asunder ; properly introduced into the circuit, fig. 8. Then, having given a small phial 2 or 3 turns of the globe, charging it positively, connect the coating of it, by a chain, with the wire of the ball A ; and on applying the knob of the phial, to the wire of the ball B, you will observe the flame to be plainly driven from it ; being often blown upon the ball A, so as to blacken it with the smoke. Then charge the phial negatively, and, the apparatus remaining as before, apply the knob of the phial as at first ; and you will then perceive the flame to be blown quite in the contrary direction, viz. from A towards, and often upon B, as on Dr. Franklin's principles of the Leyden bottle, it ought to be.

*Exper. 2.* Charge a large jar positively, and insulate it ; then take a long curved wire, pointed at both ends, and hold it by a glass handle, so as to bring one end of the wire half an inch from the knob, and the other end of it to the same distance from the coating of the jar. You will then observe a small luminous spark on the point opposed to the knob of the jar, and a fine pencil, diverging from the lower point, spreading on the coating of the jar, which will presently discharge it silently. Then charge the jar negatively ; insulate it, and apply the wire as before ; and the appearances at the points of the wire will be directly reversed ; plainly demonstrating the direction of the electricity in the discharge of the bottle.

Another very convenient and easy method, of exhibiting the phenomena of the positive and negative electricity of the inside and outside surfaces, of a charged Leyden bottle, is by slipping a cap of metal, furnished with a ball and wire, on the outside coating ; and mounting it on an electric stand, in a horizontal position, as fig. 12 ; or if the bottom of the glass be turned much upward into the body of it, a piece of wood may be worked to its shape, and cemented to it ; then through the middle of this wood, a short tube of metal may be inserted, so as to admit the wire which is connected with the ball to pass through it ; and be brought into contact with the coating of the jar, at pleasure. By this means, experiments may be made, at either end of the bottle, with great facility ; and other charged or exhausted bottles, excited ribbons, or other electrics : the curved pointed wire, &c. &c. may be readily applied ; and give or receive a spark ; be attracted or repelled ; according to the kind of electricity in the two bodies so applied towards each other. By hanging a chain round either of the wires, and connecting it with one end of the discharging rod ; and bringing the other end of the rod so as to leave a proper space between that and the ball on the wire, at the opposite end of the bottle ; the flame of a taper, &c. may be interposed, and show the direction of the electricity in the discharge : or a cork-ball, hung by silk, may play between them, in the manner described by Dr. Franklin. If the balls are taken off from the wires of the bottle, the

wires being pointed, and one of them placed before the globe, or a prime conductor, electrified positively, the phenomena of charging the Leyden bottle will be discovered by the different appearances at the ends of the wires, as at fig. 13. If the bottle be thus placed before a conductor electrified negatively; or the insulated rubber to a machine; the appearances at the ends of the wires will be reversed, as on Dr. Franklin's principles they ought to be; and thus explain his theory of the Leyden phial.

But a more simple, and beautiful analysis of the Leyden phial, hath not perhaps been exhibited, than the following. Let a bottle that will hold near a pint, having a long neck, about an inch in diameter, be furnished with a small plate at the top; with a valve properly secured, after the bottle is exhausted: from which plate, a wire about the 8th of an inch in diameter, is to project a little below the neck, and terminate with a blunt end. The top is to be covered with a round brass cap, firmly fixed on, and made air-tight. The bottom of the bottle should be coated with tin foil, which should be continued 3 inches up the side. This bottle will charge and discharge several times in a minute; and the tin foil coating will prevent the shock from affecting the hand of the operator. The phenomena of charging the Leyden bottle is elegantly explained by this contrivance, and is made visible by the end of the wire, on which the appearances vary, according as the bottle is charged, viz. positively, or negatively; or as the conductor from which it is charged, is electrified. Fig. 14, letter A, shows such a bottle, charging negatively, at a conductor loaded with positive electricity. Letter B shows the same bottle charging positively, at the same conductor. Fig. 15, letter C, shows the bottle charging positively at a conductor electrified negatively; or at the insulated rubber. Letter D shows the same bottle, charging negatively at the same conductor.

§ 3. *Of the Lateral Explosion in the Discharge of the Leyden Bottle.*

*Exper. 1.* Having made a double circuit, the first by an iron bar,  $1\frac{1}{4}$  inches in diameter, and half an inch thick; the 2d by  $4\frac{1}{4}$  feet of small chain; on discharging a jar, containing 500 square inches of coated surface, the electricity passed in both circuits, sparks being visible on the small chain in many places. On making the discharge of 3 jars, containing together 16 square feet of coated surface, through 3 different chains at the same time, fig. 16, bright sparks were visible in them all; and I have not the least doubt but it would have been visible in as many more. The chains were of iron and brass, of very different lengths, the shortest 10 or 12 inches, the longest many feet in length. When those jars were discharged through the iron bar before mentioned, together with a small chain,  $\frac{3}{4}$  of a yard in length; the whole chain was illumined, and covered throughout with beautiful rays, like bristles, or golden hair. Having placed a large jar in contact with the prime conductor, I affixed to the coating of it an

iron chain, which I also connected with a plate of metal, on which I intended to make the discharge by the discharging rod, fig. 18. This done, I hooked another chain, much longer, and of brass, to the opposite side of the jar, and brought the end of it within  $8\frac{1}{4}$  inches of the metal plate. In contact with this end, I laid a small oak stick, 8 inches long, which I covered with saw-dust of fir-wood. On making the discharge on the plate both the chains were luminous through their whole lengths; as was also the saw-dust, which was covered by a streak of light, making a very pleasing appearance. From this experiment may, I think, be inferred, the necessity of making the conductors, erected as a security to buildings, &c. from the damage of lightning, both of the best materials, and of a very sufficient substance; and for this purpose, perhaps nothing will be found so proper as lead, which will remain in the earth many centuries without any considerable decay; and the tops of chimnies being covered with it,\* and furnished with a long, sharp pointed rod of copper, or iron pointed with copper, which I think should extend at least 5 or 6 feet above the top of the chimney, or highest part of the building; a communication should be made from it by plates of lead, 8 or 10 inches broad, with the lead on the ridges and gutters, and with the pipes which carry down the rain water; which pipes should be continued to the bottom of the building, and there made to communicate, by means of another leaden pipe, or a plate of it, as before mentioned, with the water in a well, or the moist earth, or the main pipe which serves the house with water.

§ 4. *Description and Use of a new Prime Conductor.*

A, fig. 16, is a glass tube, 18 inches long, and near 2 inches in diameter. B, C, balls of brass, with a ferule, 2 inches long, to each of them; which ferules are to be cemented to the ends of the tube, and made air-tight. One of the brass plates, which are soldered to the ferules, has a small hole drilled through it, by which the air is to be exhausted. It is covered by a strong valve, properly secured, and concealed by the brass ball B or C. D, E, balls of brass, about  $\frac{1}{4}$  of an inch in diameter, fixed on wires, which project  $2\frac{1}{4}$  inches from the brass plates, at each end of the glass tube. F, a fine pointed wire, to collect the electricity from the excited glass globe, &c. G, supporters of sealing wax, on which the luminous conductor is to be mounted.

N. B. The dots in the tube are intended to represent the appearance of the electricity in it, described in the experiments. But when a bottle or a large jar is discharged through the glass conductor, it is uniformly filled with light.

\* I mention covering the tops of chimnies with lead, as a protection to the upper courses of bricks, from the effects of wind; and not as being of any essential service to the conductor, any farther than as it may assist in fixing the pointed rod, which is to be elevated above it, more securely.  
—Orig.

*The use of the Glass Conductor.*—The glass tube, thus furnished and mounted, being properly exhausted, and perfectly dry, will act in all respects like one of metal; and the electrometer, being placed on the brass ball *B*, will answer exactly to the charge of a jar or battery. But the principal use of this instrument, is to ascertain the direction of the electric matter, as it passes through it. And this end it completely answers in the manner following, viz. set it with the collecting point *F*, before the globe, and place the knob of an uncharged bottle nearly in contact with the brass ball *B*, or hang a chain, &c. from it to the table; and, on working the machine, the ball *D* in the tube becomes entirely enveloped in a dense white atmosphere of electricity. If the point *F* be brought nearly into contact with an insulated rubber, and a communication be made from the ball *B* to the table; the atmosphere will be on the ball *E* in the tube. If a bottle, positively charged, be presented as in the drawing, fig. 18, the appearances in the tube will be as there delineated. But if a bottle charged negatively, be thus applied, the atmosphere will surround the ball *E* in the tube, as in fig. 19.

If, instead of the brass balls in the tube, points are used; or if a point be fixed at one end of the tube, and a ball at the other, the effect will be precisely the same.—Note also, that the glass conductor, for the purpose of making Dr. Franklin's curious experiments, with a pointed and blunted wire, is far superior to one of metal, the electric atmosphere being so much better retained by it. By this easy and simple process, may an ocular demonstration, at all times, be given, in a dark room and dry air, of the truth and propriety of Dr. Franklin's hypothesis of the Leyden bottle.

§ 5. *Miscellaneous Experiments, made principally in the Years 1771 and 1772.*

*Exper. 1.* If a black silk ribband, or a piece of black silk, be laid on a quire of paper, &c. on a table, and excited by drawing over its surface sealing-wax, sulphur, amber, or a tube of glass with the polish taken off by emery, its electricity will be positive: whereas, if it be excited singly, or together with a white ribband, by drawing them briskly between the fingers it is always negative. Laying it on the paper, and drawing over its surface a rod, or tube of smooth glass, its electricity will also be negative.

*Exper. 2.* If a plate of glass, 10 or 12 inches in diameter, be excited, and placed on the top of a box, from which a pair of light pith or cork balls are suspended, being mounted on a stand of sealing-wax; the balls will separate, and stand repelled from each other, being electrified positively, in a dry air, upwards of 4 hours. When they come into contact, on removing the glass, they diverge again, and are negatively electrified; but on replacing it they close. On removing it again they separate; and thus alternately as long as any electricity remains in it.

If the plate of glass be placed in a frame of wood, and a light pith or cork-ball be laid on its surface, on presenting towards it the end of a finger, or the point of a pin, &c. the ball will recede from them, with a very brisk motion, and may thus be driven about on the surface of the glass, like a feather in the air, by an excited tube, or the wire of a charged bottle. The cork-ball, being deprived of its electricity by the pin, &c. instantly flies to that part of the glass to which it is attracted the most forcibly.

*Exper. 3.* I hung on the prime conductor a small phial, 2 inches in diameter, coated  $3\frac{1}{4}$  inches from the bottom. From the coating of this phial I suspended 2 chains: the first in contact with a heavy weight, placed on a card, across which I had ruled lines at equal distances, fig. 10, the 2d chain formed a circuit, with leaden pipe, small brass wire, small chain, &c. of 120 feet in length. From the ball of the discharging rod, which rested on another weight, I also hung a chain, in contact with, and completing the circuit of 120 feet before-mentioned; and observed, that if the bottle was charged quite full, the electricity would, in the discharge, pass through the long circuit, rather than over the surface of the card, when the weights were placed at  $\frac{1}{8}$  of an inch asunder; but if I charged the bottle only about half full, the electricity would, in the discharge, pass through the long circuit, rather than over the surface of the card, though the weights were placed at the distance of only  $\frac{1}{8}$  of an inch. Query, Can there be a greater proof of the small resistance made by metal to the passing of the electric matter, compared with card, wood, &c. and consequently of the utility of metallic conductors to buildings, ships, &c.? The same observation has been repeatedly made, on the effects of the natural electricity.

*Exper. 4.* Having prepared a phial, in the manner directed by Mr. Lane, for making his curious experiment, by passing a wire through the bottom, and another through the cork, so as to bring the ends of the 2 within half an inch of each other, about the middle of the bottle, which was filled with water, I found, as that gentleman observed, that a slight shock of electricity discharged through it, would break the bottle. But having put a very small wire from the top to the bottom of it, through the water; I discharged through it 3 large jars, containing 16 square feet of coated surface, when the whole of the small wire was exploded; but the bottle remained unhurt. If therefore a metallic conductor, being too small, should happen to be destroyed by a stroke of lightning, yet the building, &c. to which it is affixed, will probably escape uninjured.

*Exper. 5.* When I strongly electrify a large prime conductor, 3 feet long, and 12 inches in diameter, if a person hold in his hand a brass rod terminated by a ball, 2 inches in diameter, at the distance of 2 inches from the side of the conductor, fig. 11, he will continue to draw such strong sparks as will give him a sensible shock in both his legs; but if another person at the same time present



the point of a lancet, or a wire 5 or 6 inches long, nicely tapered to a point, tipped with steel, towards the conductor; though at the distance of 2 feet, or somewhat more, this will draw off all its electricity silently; and not suffer a spark to pass from it to the brass ball: it is also observable, that if the point of the wire, or lancet, be brought nearly into contact with the prime conductor, yet no sensation is felt in the arm, &c. of the operator. Hence appears clearly the preference due to points, rather than round balls, or blunted ends, for the termination of the conductors erected as a security to buildings, &c. from damage by lightning; for it seems probable, that the sharp point of the conductor will act on the electric atmosphere of the cloud, and perhaps gradually and silently continue to diminish the contents, before the cloud can approach near enough to strike; and thus contribute to lessen, if not actually prevent, a stroke. But should the point be struck, the consequence I suppose will not be great, and a curious instance I have now before me, which I shall beg leave to quote as follows. "About 9 o'clock we had a dreadful storm of thunder, lightning, and rain, during which the main-mast of one of the Dutch East Indiamen was split, and carried away by the deck; the maintop mast and top gallant-mast, were shivered all to pieces; she had an iron spindle at the maintop gallant-mast head, which probably directed the stroke. This ship lay not more than the distance of 2 cables length from ours, and in all probability we should have shared the same fate, but for the electrical chain which we had but just got up, and which conducted the lightning over the side of the ship; but though we escaped the lightning, the explosion shook us like an earthquake, the chain at the same time appearing like a line of fire; a centinel was in the action of charging his piece, and the shock forced the musket out of his hand, and broke the ram rod. On this occasion I cannot but earnestly recommend chains of the same kind to every ship, whatever be her destination; and I hope that the fate of the Dutchman will be a warning to all who shall read this narrative, against having an iron spindle at the mast head." See Capt. Cook's voyage. This conductor was of copper wire,  $\frac{3}{8}$  of an inch in diameter; which I am inclined to think is rather too small for the purpose; I am of opinion it ought to be a quarter of an inch at least; as I have been informed by Dr. Solander, that the point originally belonging to the conductor, had been stolen; and that this, on which the lightning fell, was of inferior workmanship, and not so sharp; which was another great disadvantage: perhaps if the wire of the chain had been larger, and the point more acute, the stroke would have been much lessened, if not absolutely prevented. If, instead of those chains, plates of copper,  $\frac{3}{8}$  of an inch thick, and 2 inches broad, with the edges neatly rounded off, were inserted in a groove, and continued down the maintop-gallant-mast, the maintop-mast, and part of the main-mast, into the well-hole: a communication from the mast, to the underside of one of the decks,

might be made with a plate, or rod of metal, flattened at each end; and from that rod the conductor might be continued by plates of lead, or copper, on the underside of the deck, and down both the outersides of the ship, as low as the keel, if it be thought necessary: and this method I should apprehend would be preferable to the chains, which are now in use. Particular care should be taken, to have all the plates, which form the conductor, as nearly as possible in contact with each other, and to fix a sharp pointed slender rod of copper at its summit. And for the purpose of connecting the plates, inserted in the maintop-gallant-mast, the maintop-mast, and the main-mast; if a hoop of copper were fixed in a groove of its own thickness, at the top of the main-mast; and another such hoop at the upper end of the maintop-mast; perhaps they might answer this end very conveniently. Dr. Watson has collected from ancient history, the accounts of electrical appearances, on pointed bodies; as the spears of soldiers, &c. &c. which have been very judiciously introduced by Dr. Priestley into his *History of Electricity*; and I cannot but think those accounts furnish a very strong argument in favour of pointed conductors; for had the bodies here spoken of been terminated by blunted ends, or round knobs, it is probable that many of them, instead of drawing off the lightning silently, would have been struck with it; and this, being deemed a common occurrence, would have passed unnoticed, and consequently never have been recorded in history.

If pointed bodies had really the property of drawing down strokes of lightning on themselves, I think the pillar on Fish-street Hill, commonly called the Monument, could not long have escaped. This pillar is terminated by a basin of metal, 4½ feet in diameter. The basin is surrounded by a great number of bended plates of metal, sharply pointed, to represent flames of fire. From the basin, to the floor of the gallery, are fixed perpendicularly in a circular order 4 thick bars of iron; and in these bars are inserted 28 strong hoops, and 4 segments of circles, of the same metal, which serve as steps from the gallery to the basin. One of these bars, being 1 inch thick and 5 inches broad, is connected with the iron rails of the stair-case, which reaches to the bottom of the building, and forms a substantial, regular conductor of metal the whole length. The monument was erected by Sir Christopher Wren in remembrance of the fire of London, which happened in the year 1666. It was completed by that great architect in the year 1677; is, including the blazing urn at its summit, about 202 feet in height, from the pavement; and has never, as far as I have been able to learn, been struck by lightning. The antennæ and legs of the grasshopper on the Royal Exchange in Cornhill; and the tongue and tail of the dragon on the spire of Bow church in Cheapside, London, are also remarkable instances: indeed I have often thought it rather a favourable circumstance, that most of the lofty public buildings in this metropolis which have metallic termi-

nations, have generally been furnished with weather-fanes, which commonly end in sharp points; for had they been terminated with large round balls of metal, perhaps many more of them might long since have been demolished. Here therefore I cannot but express my earnest wishes, that on all future occasions, where lofty public edifices are to be erected, a good pointed conductor for the lightning, may be considered by every architect, or surveyor, as an essential part of the edifice itself.

*Exper. 6.* I attempted to ascertain the conducting power of different metals, in the following manner. I took a thick piece of paste-board, across which I ruled lines, exactly an inch asunder. On these lines crosswise I placed the wires, which were confined by heavy weights: the edges of which weights just touched the ruled lines; leaving exactly an inch of wire between them (see fig. 10). The kinds tried were, pure gold, silver, brass, copper silvered, and iron. They were all drawn through the same hole, except the iron, which was somewhat larger than the others. They were proved by 2 jars, containing 11 square feet of coated surface; and the charges were adjusted by an electrometer graduated in divisions of a 10th of an inch each, the diameter of the scale being 2 inches. The result was as follows:

Pure gold	}	was melted at	{	4	}	divisions.
Brass				6		
Copper silvered				8		
Pure silver				10		
Iron				10		

When I gave either of the wires a division less than the number above specified, it was not melted; when I gave either of them a division more, it was exploded; the greater part vanishing in smoke; whereas these charges just burst them into balls.

Having lately been presented, by Dr. Lewis, with 6 specimens of his platina, in as many different states, I selected the largest grains, from one of the parcels which he informed me had been repeatedly exposed to long-continued vehement fires; the most intense he had been able to excite, or any vessels he could procure would support: and after a few small globules, consisting doubtless in great part of heterogeneous metal, had melted out, repetitions of the operation produced no further change. It was afterwards boiled successively in oil of vitriol, aquafortis, and spirit of salt, in order to its further purification; and which indeed reduced it to a state the most pure of any that excellent chemist had been able to produce. Having ruled a line with a blunt ended wire, over the surface of a plate of white wax;

*Exper. 7.* I pressed in the grains of platina lightly, and in contact with each other, so as to form a regular line, half an inch long. At each end of the line of platina, and in contact with it, I placed a thick wire, with its ends nicely

rounded off, and made perfectly smooth. I covered the platina with a piece of thick plate-glass; and then discharged through it, 3 jars containing 16 square feet of coated surface: when I obtained many beautiful spherules of the platina. Several of them stuck to the wax and glass; and others imperfectly formed, on the edges, &c. of the grains; which proved that the fusion had been complete.

*Exper. 8.* I made a long cork perfectly dry, and held one end of it very near the fire, till it began to burn. At the same time I held a small fine toothed file in the clear part of the fire, till that also had become very dry, and rather hot. Then, having filed off the end of the cork, I applied it to a pair of neat light pith balls; when it attracted them both, and raised them perpendicularly, as high as the strings would permit. Having electrified the balls by excited amber, the cork would increase their divergence from 1 to near 2 inches; or it would repel them at an inch distance, so as to drive them  $1\frac{1}{4}$  inch out of the perpendicular. Electrifying the balls by excited glass, these appearances were directly reversed. The cork therefore had parted with its electricity to the file, and plainly acted as a negative electric.

*Exper. 9.* Having neatly rounded off the corners of a piece of thin talc, about 3 inches square; I coated both its sides within  $\frac{1}{4}$  of an inch of the edges, with tin-foil, which I also rounded off at the corners. The talc, thus prepared, I observed would readily charge, without wiping, or drying the uncoated part, and the force of the shock, in the discharge, was really astonishing.

Having been shown, by my late truly ingenious friend Mr. Canton, an electric spark, of a very beautiful crimson colour, which always appeared as it was drawn over, or through, a piece of smooth wood, at the top of the conductor-stand, and which was supposed by some gentlemen to be the light of electricity, very thinly spread on the surface of the wood; I was exceedingly desirous to know from what cause this phenomenon really proceeded; and for that purpose made the following experiment.

*Exper. 10.* I fixed between 2 balls, introduced into the circuit of an electric discharge, a piece of smooth wainscot, about 2 inches in diameter, and a quarter of an inch thick; when, on making the discharge of a pretty large jar, I observed the wainscot to be nearly covered with the electric light, the outer parts, or edges of the light, were exceedingly thin, but the colour very white, as it was also in several other experiments, made with the same intent. I then procured a circular piece of coloured box, which was glued to the top of the stand to my prime conductor; when, drawing strong sparks through this wood, of whatever colour it was, I became clearly of opinion, that the colour of the spark varied according to its depth in the wood, viz. if it passed on the surface, it was white; a little below it, yellow, or orange: still lower, scarlet: and deeper in the wood, crimson.

It having been mentioned by some gentlemen, as their opinion, that the matter of light, and the electric matter, were the same thing; I made the following experiment, in order to determine whether there was any foundation for such an opinion.

*Exper. 11.* I insulated the rubber of my machine, and placed it in such a situation, that the rays of the sun, passing through the open window of my room, might fall immediately on it; but this I observed produced no electricity. I then collected the rays into a focus, by means of a good convex glass, and threw them on the back of the rubber, till it was burned quite black; but this method was attended with no better success. I then mounted one of Mr. Canton's electrometers, furnished with very light balls, on a stand of sealing-wax; and having electrified them negatively, by excited amber, so as to diverge a full inch, I again collected the rays of the sun by the convex glass, and held it at such a distance as to bring the focus exactly on the end of the box, which was burnt very black, and the glue in the joints melted; but the balls were not in the least affected.

*Exper. 12.* Hold a piece of amber near the flame of a candle, till it becomes hot: then apply it to a suspended thread, and it will not attract it, neither will it become electrical in cooling; but press it ever so lightly on your hand, in order to try its heat, though without the least friction, and, if it be not too hot, it will be electrical, and attract it violently. Heat it again at the candle, and its electricity shall be taken quite away. Press it again gently on your finger, or hand, and the power will be restored. Apply it again to the candle, it is lost. And thus alternately. Other electrics may probably act in the same manner; as the flame of a candle, or hot air, will conduct away the electricity of glass, almost instantaneously.

*Exper. 13.* Showing Mr. Nairne the above-mentioned experiments: when the amber had been well heated, and being presented to a suspended thread, having shown no sign at all of electricity; I held it, between my thumb and fore-finger, very near the table, but not so as to touch it, that we might entirely avoid friction. He then blew against it 30 blasts, with a pair of kitchen bellows; when presenting it to the thread it attracted it, at the distance of  $\frac{1}{4}$  of an inch. He then blew against it 30 blasts more, as above described; when applying it again to the thread, we saw it attracted at half an inch distance; and on drawing back the amber, it drew the thread after it 6 or 8 inches. We repeated the experiment 3 times with the like success; and are satisfied, that the amber was made electrical by the friction of the particles of air against its surface: and not in the least by heating only. We afterwards excited the amber, when it must have been perfectly cold, but dry, by only blowing against it as before. The same process succeeds with glass.

§ 6. *Experiments and Observations on the Electricity of Fogs, &c.*—1771, Nov. 14, half past 8, A. M. I find a fog, not very thick, pretty strongly electrified. The balls separate full half an inch. They keep stationary, there being little or no wind.

Dec. 2, half past 8, A. M., a fog, moderately thick, is strongly electrified. The balls diverge half an inch; but when they are brought near the building, they close, and open again on removing them. The mercury in the thermometer is 15 degrees above the freezing point.

Dec. 18, half past 4, P. M., a moderately thick fog is strongly electrified soon after its appearance. The balls diverge full half an inch, and regularly close at the approach of excited wax. The wind is troublesome, but the balls keep their distance, and at intervals very well admit trying the experiment.

1772, Jan. 5, a fog is strongly electrified positively. The balls diverge full half an inch. The air is sharp, and frosty.

Jan. 13, 9 o'clock, A. M., a fog, not very thick, is strongly electrified positively. The mercury in the thermometer is  $7\frac{1}{2}$  degrees above the freezing point. There is little or no wind.

Jan. 18, 10 o'clock, A. M. The air is pretty strongly electrified by a fall of snow.

From the small number of experiments I have been able to make on the electricity of the atmosphere, I cannot help being of opinion, that fogs are much more strongly electrified in, or immediately after, a frost, than at other times; and that the electricity in the fogs is often the strongest, soon after their appearance. I also now hold it for a certain rule, that whenever there appears a thick fog, and the air is at the same time sharp and frosty, that fog is strongly electrified positively. Though rain may not be an immediate, yet I am inclined to think it is by no means a very remote consequence of electricity in the atmosphere; and, from the trifling observations I have had an opportunity to make on that subject, I have not failed to find that in 2 or 3 days after I have discovered the air to be strongly electrified, especially if that electricity continued for as long a time, we have had rain, or other falling weather, and I incline to believe, more plentifully in proportion to the strength and continuance of the electricity; if not rain, snow, &c. according to the state of the atmosphere, with respect to heat and cold. If electricity be not a cause, I think it at least a prognostic, of falling weather.

*XLII. A Letter from David Macbride, accompanying a Letter from Mr. Simon to Dr. Macbride, concerning the Reviviscence of some Snails preserved many Years in Mr. Simon's Cabinet.* p. 432.

In Mr. Simon's letter of the 20th of November, he mentions a particular

shell, whose snail had come out 4 several times, in the presence of different people, each of whom have assured me that they saw it. A day or two after the date of that letter, the above gentleman brought the identical shell, as he declared, into the presence of several other persons, that they might try if the snail would again make its appearance. The company were not disappointed: for after the shell had lain 10 minutes in a glass of water that had the cold barely taken off, the snail began to appear: and in 5 minutes more we perceived half the body fairly pushed out from the cavity of the shell. We then removed it into a basin, that the snail might have more scope than it had in the glass: and here in a very short time, we saw it get above the surface of the water, and crawl up towards the edge of the basin. While it was thus moving about, with its horns erect, a fly chanced to be hovering near, and perceiving the snail darted down on it. The little animal instantly withdrew itself within the shell, but as quickly came forth again, when it found the enemy had gone off. We allowed it to wander about the basin for upwards of an hour; when we returned it into a wide mouthed phial, where Mr. Simon had lately been used to keep it. He presented me with this remarkable shell; and I observed, at 12 o'clock, as I was going to bed, that the snail was still in motion; but next morning I found it in a torpid state, sticking to the side of the glass.

In a few weeks after the time abovementioned, I took an opportunity of sending this shell to Sir John Pringle, who showed it at a meeting of the society; but as he has been pleased to inform me, some of the members could not bring themselves to believe but that Mr. Simon must have suffered himself to be imposed on by his son, who, as they imagined, substituted fresh shells, for those which he had got out of the cabinet. On this, I wrote to Mr. Simon, which produced his letter of the 4th of February. I afterwards also examined the boy myself; and could find no reason to believe that he either did or could impose on his father.

Mr. Simon is a merchant of this place, of a very reputable character, and undoubted veracity. He lives in the heart of the city, a circumstance which rendered it almost impossible for the son (if he had been so disposed) to collect fresh shells. The father of Mr. Stuckey Simon was Mr. James Simon, F. R. S., who, being a lover of natural history, as well as an antiquarian, made a little collection of fossils, which is still in the son's possession, and contains some articles that are rather uncommon.

*Mr. Stuckey Simon to Dr. Mackbride, dated Dublin, Nov. 26, 1772.*

SIR,—An accident having brought to light what some naturalists have not had an opportunity to examine into, and which has been a subject of some conversation among gentlemen to whom I have mentioned it, has made me commit to writing the simple facts, in order to put others on making further experiments on the subject. — About 3 months since, I was settling some shells in a drawer;

among which were some snail shells. I took them out, and gave them to my son, a child about 10 years old, who was then in the room with me. The Saturday following, the child diverting himself with the shells, put them into a flower-pot which he filled with water, and next morning put them into a basin. Having occasion to use it, I observed the snails had come out of the shells. I examined the child. He assured me they were the same I gave him some days before; and said he had a few more, which he brought me. I put one of them in water; and, in half an hour after, I observed it put out its horns and body, which it moved with a slow motion, I suppose from weakness. I then informed Major Vallancy and Dr. Span of this surprizing discovery. They did me the favour to come to my house the Saturday following, to examine the snails; and, on putting them in water, found that only one had life, which was that I put in water, for it came out of its shell, and carried it on its back about the basin. The rest I suppose died by being kept too long in water; for, on the first discovery, I let them remain in the water till the Monday following, when I poured off the water, the snails being still out of their shells, and seemingly dead. They lay in that state till Tuesday night, when I found they had all withdrawn into their shells; and, though I several times since put them into water, they showed no signs of life. Dr. Quin and Dr. Ruty did me the favour, at different times, to examine the snail that is living; and were greatly pleased to see it come out of its solitary habitation, in which he has been confined upwards of 15 years, for so long I can with truth declare it has been in my possession; as my father died in January 1758, in whose collection of fossils those snails were, and for what I know they might have been many years in his possession before they came into my hands. The shells are small, and of one kind; white, striped with brown.—Since this discovery, I have kept this snail in a small phial, with a cover with holes, to let in air; and it seems at present very strong, and in health.

*XLIII. The Bill of Mortality of the Town of Warrington, for the Year 1773.*

*By the Rev. J. Aikin. p. 438.*

The town of Warrington, contains between 1600 and 1700 houses. At 5 persons to a house, which is supposed a sufficient allowance, as but few are occupied by more than one family, this will give above 8000 for the number of inhabitants. The average of yearly marriages, christenings, and burials, registered in the parish church,

Marriages. Christenings. Burials.

From 1750 to 1769 inclusive, is . . . . . 73 . . . . . 237 . . . . . 199.

For the years 1770, 1771, 1772, is . . . . . 95 . . . . . 331 . . . . . 258

This will serve to show the increase of the place, and its comparative health.



ness; especially if we consider that the deaths are much more exactly registered than the births. In the present bill, the number of children, who died after receiving only private baptism, in consequence of which their deaths were registered, but not their births, amounts to 17; which might therefore be added to the average of christenings for the last 3 years, and will form an extraordinary instance of healthiness and increase. The present bill also takes in the separate registers kept by different societies, in which the births much exceed the burials, as many of the latter are entered at the parish church.

The melancholy overbalance of burials, which now appears, plainly arises from the dreadful ravages of a single disease, the small-pox; which perhaps has seldom raged with greater malignity than in its late visitation of this town. Its victims were chiefly young children; whom it attacked with such instant fury, that the best-directed means for relief were of little avail. The state of the air went through all possible variations in the course of it, but with no perceptible difference in the state of the disease. In general, the sick were kept sufficiently cool, and were properly supplied with diluting and acidulous drinks; yet where they recovered, it seemed rather owing to a less degree of malignity in the disease, or greater strength to struggle with it, than any peculiar management. Where it ended fatally, it was usually before the pustules came to maturation; and indeed in many they showed no disposition to advance after the complete eruption, but remained quite flat and pale. In one neighbourhood, out of 29 who had the disease, 12 died, or about 2 in 5; in others the mortality was still greater, and there is reason to believe it was not less on the whole. It may perhaps be worthy of observation that the proportion of females who died, to males, was nearly as 3 to 2. While we lament the severity of the scourge with which we have been afflicted, we cannot but highly regret, that a practice, which experience has established as so effectual a security against it, was so little followed. Not 10 were inoculated in the whole town and neighbourhood: these all did well, yet their example was not sufficient to overcome some accidental prejudices taken against it.

*General Bill for 1773.*

Marriages.	Births.	Burials.
93	Males 175 Females 181	Males 223 Females 250
	} 356.	} 473.

Of these 473 deaths, 211 were by the small-pox.

*XLIV. Of the Stilling of Waves by Means of Oil. Extracted from sundry Letters between Benj. Franklin, LL. D., F. R. S., Wm. Brownrigg, M. D., F. R. S., and the Rev. Mr. Farish. p. 445.*

This paper may be consulted in Dr. Franklin's works, collected and published in 1806, in 3 vols. 8vo. see p. 144, vol. 2.

*XLV. On a New Map of the Northern Archipelago, and a Specimen of Native Iron. By M. de Stehlin, Couns. of State to her Imperial Majesty of Russia. p. 461.*

As a testimony of his attachment to the R. S., and as the first tribute he owed to that learned body, he had the honour to transmit herewith 2 novelties, which he thought worthy of their notice. The first was a new map, and his preliminary description of a new Archipelago in the North, discovered a few years before by the Russians, in the N. E. beyond Kamtshatka. The second was a piece of raw and native iron; of which Mr. Pallas, one of the R. S. of Petersburg academicians, who had 5 years been employed in making researches in natural history, in the provinces of the Russian empire, had discovered in 1773 a hillock or mass, weighing 50 puds, the pud consisting of 40 Russian pounds, in Siberia, in the mountains called Nemir, between the rivulets Ubec and Sisim, which fall into the river Jenisei, scarcely 100 fathoms from a rich mine of loadstone or iron.

The existence of raw or native iron has hitherto been doubted; but M. de S. almost thinks that this discovery determined the question; especially when it is considered, that in the whole district where this mass was found, there is not the least trace extant of any ancient forge, nor any place that might leave room to suspect that there had been, in former times, any works of iron ore, which had been melted, and afterwards abandoned to that mass. Should any doubt remain concerning the existence of the native iron, and the authenticity of this discovery, he should rather suppose that, many ages ago, there might have been a volcano, which by melting the iron ore had formed the above mass, to which might afterwards have been joined the little hyacinthine spars and other stones now mixed with it.

*Translation of an Article in the Petersburg Gazette of Sept. 6, 1773.*

“The academy expects from Siberia a black mass weighing about 40 puds,\* of raw or native, soft and flexible iron, which the academician Pallas has discovered during his residence in the neighbourhood of the river Jenisei. This very remarkable and huge lump is of a spongy texture, of the most perfect and malleable iron, whose cavities are closely filled with small polished pieces of hyacinthine spar, some round, some with flat surfaces, and all of the colour of transparent amber. The mass is rusty only on the surface; but the interior has been preserved by a kind of black varnish spread all over the iron, which is of an irregular form blunted at the corners. This iron may be bent and hammered when cold, and, when moderately heated, may be shaped into nails and other tools; but, in a violent heat, and especially if, in order to separate it

\* The mass in its present state, weighs 152 Russian pounds.

from the sparry particles, it is thrown into smelting ovens, it becomes brittle, granulated, and will not join again in the forge.

This mass was found lying on the surface, at the top of a high woody eminence, not far from the mountains called, by the Tartars, Nemir, between the two rivulets Ubei and Sisim, which fall from the right into the Jenisei, a little below Abakanskoi Ostrog, and scarcely 100 fathoms from a rich mine of hard ore of loadstone. The appearance and nature of this mass, and the qualities of the iron of which it chiefly consists, are so decisive, that it cannot be doubted but that it has been thus produced by nature; and if so, the existence of native iron, which has hitherto been questioned, is established beyond all contradiction; especially if it be considered, that no trace of any old iron work, of which there are many in the Siberian mountains, is to be met with in the desert where the mass was found; and the mine abovementioned was not opened before the year 1752, when the miners, who were there employed, first discovered this mass of iron: since which time no further notice had been taken of it."

*XLVI. Of Torpedos found on the Coast of England. By John Walsh, Esq.  
F. R. S. p. 464.*

"It has lately been found, that the torpedo, or electric ray, frequents the shores of this island, contrary to a received opinion among naturalists, who have in general considered it as an inhabitant only of warmer climates. In consequence of inquiries Mr. W. had set on foot in some of our southern fishing ports, 2 torpedos, taken in Torbay, one in the beginning of August, and the other in the beginning of Nov., last year, (1773), have been actually sent up to this metropolis. The first, procured by the good offices of Mr. Amyatt, apothecary, in Berkeley-square, was examined, and the electrical organs were successfully injected, by Mr. John Hunter. The second, forwarded by Mr. Grant, a principal fishmonger in the land carriage branch, then at Brixham, came up very fresh and perfect, in one of his fish machines. This was weighed and measured before it was touched by the dissecting knife, and found to weigh 53 lb. avoirdupois, and to measure 4 feet in length, 2 $\frac{1}{4}$  feet in its extreme breadth, and 4 $\frac{1}{2}$  inches in its extreme thickness.

The largest torpedo Mr. W. met with in the neighbourhood of Rochelle, where upwards of 70 passed through his hands, weighed little more than 10 lb. and measured not quite 2 feet in length, nor quite 16 inches in breadth: and the largest he had read of was that mentioned by Rhedi to Lorenzini, weighing 24 lb. doubtless of Leghorn, which make about 18 avoirdupois. Though this Mediterranean torpedo has been ever considered as of an extraordinary size, it is exceeded in weight nearly 3 to 1 by our enormous British torpedo.

Its back was of a dark ash colour, with somewhat of a purple cast, but not at all mottled like those of the Atlantic coast of France, nor regularly marked with eyes, as they have been called, like some found in the Mediterranean. Its under part was white, skirted however with the same ash colour, which towards the tail became almost universal. The side fins, being a little contracted and curled up, prevented the precise measurement of its breadth, but it appeared to hold the general proportion observed in those of Rochelle; that is, the breadth was  $\frac{1}{3}$  of the length. Its electric organs likewise were proportionate with theirs, each organ measuring 15 inches in extreme length, and 8 in extreme breadth. In short, the torpedo of Torbay no way differed from those seen in the Bay of Biscay, but in size and colour: and perhaps this difference may be thought rather casual than denoting a specific distinction.

It was a female, without any signs of pregnancy. The intestines contained, with some black slime, 2 vertebrae of a fish, seemingly of the cod kind. The electric organs of this torpedo were likewise injected by Mr. Hunter, though not with his first success, from the bursting of the artery in the operation; he determined however the number of columns, in one organ, to amount to 1182, and fully confirmed the observation he formerly made, that their numerous horizontal partitions were very vascular.

The frequent, and perhaps favourite situation of the torpedo, is to lie in concealment under sand. If it be placed by design, as it is sometimes left by accident, in any hollow of a sandy beach, whence the tide has just retired, it swims to that brink where the water is still draining away, and on finding itself unable, after repeated attempts, to push itself over the shallow, and follow the course of the tide, it begins with admirable address to bury itself in the sand, and by a gentle but quick flapping of its extremities all round, soon sinks itself a bed, and in the action throws the sand in a light shower over its back. Neither the animal nor the spot it is in can now be distinguished; save only that, on a nice search, its two small inspiratory foramina, and their membranes at play, may be perceived. It is in this situation that the torpedo gives his most forcible shock, which throws down the astonished passenger who inadvertently steps on him.

Mr. W. has thus shown that Great-Britain too claims the torpedo, or electric ray; that ours is the broad marine sort, which Socrates, as Meno thought, resembled; and that it is the black torpedo, whose influence subdues obstinate headaches, and the gout itself.\* In announcing to our naturalists and electricians the presence of this wonderful guest, Mr. W. says, he should certainly felici-

\* Scribonius Largus, cap. 1, and 41. See also several of the early physicians, Roman and Arabian, for different cures attributed by them to the effect of the torpedo.—Orig.

tate our individuals on their acquisition, but that the Leyden Phial contains all his magic power.

*XLVII. Description of a Double Uterus and Vagina. By John Purcell, M. D.  
Professor of Anatomy in the College of Dublin. p. 474.*

The body of a woman, who had died in labour in the 9th month of her pregnancy, was dissected at the anatomical theatre of Trinity College. In the summer of 1773, on opening the abdomen, a uterus appeared of such a size and form, as are generally observed at that period. It contained a full grown foetus; but was furnished with only one ovarium and one Fallopian tube, which were situated on the right side. On the left was placed a 2d uterus unimpregnated, and of the usual size, to which the other ovarium and tube were annexed. But these 2 uteri were totally distinct and separated from each other, except at the lower extremity of their necks, where their union extended  $\frac{1}{4}$  of an inch, and an acute angle was formed between them. There was nothing extraordinary in the formation of the external parts of generation; but from each side of the meatus urinarius a membrane ran downwards; and the two, having comprehended this orifice between them, were joined together a little below it, so as to form, by their union, a septum or mediastinum, which taking the remainder of its origin from all that prominent ridge called the superior columna, and descending perpendicularly, was inserted into the inferior columna, so as to extend from the entrance of the vagina as far backward as its posterior extremity, and thus to divide it into two tubes of nearly equal dimensions. But each of these did not lead solely to the womb of its own side; for the right vagina became gradually wider as it ran backward, and at last was so far dilated as to comprehend, within its circumference, the orifices of both uteri; while that on the left side, having taken an oblique direction, ended in a cul de sac, or cæcum. Such a confirmation might have rendered it totally useless: to prevent which, nature, fertile in expedients, seems to have had recourse to a very extraordinary contrivance. This was a fissure in the septum, an inch in length, and about an inch distant from the womb of that side. Though its circumference was perfectly smooth, we must acknowledge that it might have arisen from an accidental rupture of the septum; the lips of the wound not uniting, and, in process of time, becoming callous; and yet, he imagined, that the parts were originally formed in this manner, in order to preserve a communication between the 2 vaginæ.

Thus it appears, that both uteri might be impregnated through either vagina, as that on the right side led directly to both; and as, by means of the fissure in the septum, the semen could easily be thrown from the left vagina into the right, where the apertures of the 2 wombs were placed. Through the latter

passage both uteri would seem to have an equal chance for impregnation; for, notwithstanding that which contained the foetus was placed almost directly in a line with the axis of the right vagina: yet this probably was not its original position; but by degrees its bulk increased so much as necessarily to occupy the middle space, and push the unimpregnated one aside. But however surprising it may seem at first view, yet there was reason to imagine, that the right womb, though at a greater distance, would be much more apt to conceive than the other, if the left vagina only had been made use of. For when this was distended, it appeared that the posterior part of the septum, by its protuberance, closed up and covered the left os tincæ; and as such would probably be the case in copulation, the semen not finding a ready admission into it, would pass over to the right orifice, where its entrance could not be so much obstructed. So that, if he may hazard a conjecture, he thought it more likely, since the right uterus alone conceived, that the left vagina had generally been employed.

It was a prevailing opinion among the ancients, that male children were conceived in the right side of the womb, and females in the left. Having so few opportunities of dissecting human subjects, they depended too much on the analogy of the structure of brutes, which has been the principal source of the many erroneous descriptions met with in their works. It is well known that the uterus of many quadrupeds is divided into 2 cornua, in which the foetuses are lodged; and it was not very absurd to conclude, that nature might have formed them for the distinct repositories of the 2 sexes. Accordingly this was supposed to take place in the human uterus, which has been described and delineated as if distinguished into 2 chambers. Hence arose the opinion, which is received in some places to this day, that a very sure prognostic, with regard to the sex of the child, may be drawn from the side of the belly on which the tumour is more sensibly felt. Dissections, being now more frequent, have proved, that the human womb generally has only one undivided cavity; so that the foetus, let it come from which tube it may, will, when arrived to a certain size, occupy it entirely. This observation however is not sufficient to refute the supposition that each sex might have its peculiar ovarium; and some authors pretend, that they are able to determine how many males or females any animal has brought forth, by examining the number of cicatrices on its ovaria. For, when females only had been produced, the right ovarium was found still full of vesicles, but the left quite exhausted. That this is not always the case in brutes, appears from the observation of Dr. Harvey, who frequently found male foetuses in the left cornu, and females in the right. In the human subject, opportunities of ascertaining this matter must occur very seldom. We have an instance, recorded by Cyprian, where both a boy and a girl were conceived, though the right tube was wanting. But the present case affords another example, which is

decisive; for here the impregnated uterus had not the smallest communication with the left ovarium or tube, and yet it contained a female foetus.

The septum was not merely membranous, but fleshy, and of a considerable thickness; and, like most other mediastina in the human body, consisted of 2 laminæ combined. Of these each vagina furnished one; for each had its own constrictor, and being completely surrounded by muscular fibres, had a power of contraction independent of the other, which could not be effected if both vaginæ were comprehended within the same muscular rings, and separated by a membrane incapable of action.

It has been the opinion of many modern authors of the first reputation, that the fundus is that part of the womb, whose extent increases, in the greatest proportion during pregnancy; and on this supposition they have founded various theories. One of the principal arguments which they propose, in support of their opinion, is, that the insertion of the Fallopian tubes is removed from the angles of the uterus, and gradually descends towards its neck; so that a short time before delivery they are at a very great distance from their former position. Haller does not attempt to deny these facts; but mentions 3 instances where the tubes did not change their place. But Petit, in his *Memoire* on the cause and mechanism of child-birth, is clearly of opinion, that the whole doctrine is destitute of foundation. He asserts, that the fundus increases less than any other part, and that the surprizing growth of the womb is effected by fresh supplies of fibres, successively furnished by the neck and parts adjoining. As a decisive proof, he insists that the insertion of the tubes continues nearly in the same place, and accounts for the error of the abovementioned authors by observing, that as the fundus is pushed upwards by the growth of the other parts, a greater portion of the tubes will adhere to the surface of the womb, and thus the apparent place of insertion be very far distant from the real one. This remark is verified in the present instance; for the tube at first sight appeared to penetrate into the middle of the uterus; but on a closer inspection, and by introducing a bristle, it was found to run for a considerable space between it and the coat which it receives from the peritonæum, and at length to enter into its cavity, not very far from the spot which it may be supposed to have occupied before impregnation.

With regard to superfœtation, it is evident how easily it might have been effected in the present subject; and the supposition of a double uterus can readily account for it on many other occasions. But this is a matter on which it would be needless to dwell any longer, as it has been very fully treated in Gravel's Dissertation, published in Haller's Collection; where we meet with a similar instance of 2 uteri and a vagina, the anterior part of which was divided by a septum, but whose posterior portion was single, where the septum was discontinued.

Haller, in his *Opuscula Pathologica*, gives the history of a young lady of quality who had 2 wombs, each of an oval shape, and furnished with its own peculiar vagina. One of these vagina was anterior, and communicated with the right womb; the other was posterior, and led to the left. And it is worth observing, that in these two cases, and in most others of the same kind, which have been hitherto observed, each uterus had only 1 ovarium, and 1 tube.

A double uterus is described by O. Acrel, in a treatise printed at Stockholm, in 1762; and in the 7th vol. of Haller's *Elementa Physiologiæ*, various authors are referred to, who deserve to be consulted on this subject. In some of these we find examples of 2 wombs, or 1 uterus divided into two cornua. In other instances the uterus retained its proper external appearance, though it was really double, its cavity being divided by a septum.

Since therefore it is certain that, in the structure of the parts of generation, Nature frequently deviates from her ordinary course, practitioners in midwifery ought to consider how many difficulties they may perhaps be exposed to, by not attending to the possibility of sometimes meeting with those organs formed in the same manner as in the subject of this essay. An attention of this kind would probably have been of the utmost consequence in the present case; for the orifice of the unimpregnated uterus was so far dilated, as easily to admit 2 fingers, which might have arisen from the attempts of the midwife to bring on delivery: nor can we conceive any thing more vexatious than such a case would prove, were it to fall into the hands of an inexperienced person; as the orifices of the different wombs presenting themselves alternately to his touch, he might entertain doubts of the pregnancy of his patient, even when her labour was approaching; and, by endeavouring to dilate the left vagina, all his efforts to promote delivery, would only serve to render it more difficult, or perhaps impracticable.

*XLVIII. On some Specimens of Native Salts, collected by Dr. Brownrigg, and shewn at a Meeting of the R. S., June 23, 1774. p. 481.*

This paper contains a description of some specimens of native salts, mentioning at the same time the places where they were found.

END OF THE SIXTY-FOURTH VOLUME OF THE ORIGINAL.

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*I. Experiments on the Torpedo, made at Leghorn, January 1, 1773. By Dr. John Ingenhousz,\* F. R. S. Anno 1775. Vol. LXV. p. 1.*

As I could get no torpedos alive to my lodgings at Leghorn, I hired a fishing

\* Dr. Ingenhousz, was a native of Breda, and for some time practised physic in his native country. About the year 1767 he came to England, to learn the Suttonian method of inoculating the



vessel, called a tartana, with 18 men, and went out 20 miles to sea, where the bottom is muddy, and where those fish are chiefly to be found. We caught 5; of which 4 were about a foot in length, and the other of a smaller size. Before the nets were taken up, I charged a coated jar by a glass tube, and gave a shock to some of the sailors, who all said they felt the same sensation as when they touched the torpedo. They also said, that this animal has but very little force in winter, and cannot live a long time out of the water. I put the torpedos immediately into a tub, filled with sea water, together with 2 or 3 other fishes, which I found not at all hurt by their company. I took one of the torpedos in my hand, so that my thumbs pressed gently the upper side of those two soft bodies at the side of the head, called (perhaps very improperly) *musculi falcati* by Redi and Lorenzini, while my forefingers pressed the opposite side. About a minute or 2 after, I felt a sudden trembling in my thumbs, which extended no farther than my hands: this lasted about 2 or 3 seconds. After some seconds more, the same trembling was felt again. Sometimes it did not return in several minutes, and then came again, at very different intervals. Sometimes I felt the trembling both in my fingers and thumb. These tremors gave me the same sensation as if a great number of very small electrical bottles were discharged through my hand very quickly one after the other. The fish occasioned the shock, or trembling, as well out of the water as in it. The shock lasted sometimes scarcely a second; sometimes 2 or 3 seconds. Sometimes it was very weak; at other times so strong, that I was very near being obliged to quit my hold of the animal. The torpedo having given one shock, did not seem to lose the power of giving another of the same force soon after; for I observed several times, that the shocks, when they followed one another very fast, were stronger at last than in the beginning; and this was the same when the fish was under water as when kept out of it. The pressure of my fingers, more or less strong,

small-pox; and in 1768, on the recommendation of Sir John Pringle, he was engaged to go to Vienna to inoculate the Archduchess Teresa-Elizabeth, daughter of the emperor Joseph II., and his majesty's two brothers, the archdukes Ferdinand and Maximilian; and the next year he went to Italy and inoculated the grand duke of Tuscany. The rewards of these services were the rank of body physician and counsellor of state to their Imperial Majesties, with a pension for life of about 600*l.* sterling per annum. For many years afterwards he resided chiefly in England, almost unceasingly employed in scientific pursuits, till the time of his death, which happened at Bowood Park, the seat of the marquis of Lansdowne, Sept. 7, 1799, at a very advanced age. Dr. I. was a man of great simplicity of manners, and benevolence of disposition; to whom the public are indebted for several curious and useful discoveries; particularly in the application of pneumatic chemistry and natural philosophy, to the purposes of medical and agricultural improvements. Besides several ingenious papers in the *Philos. Trans.* from vol. 65 to vol. 70, Dr. I. published in 1779, "*Experiments on Vegetables, discovering their great power of purifying the common air in sunshine, and of injuring it in the shade and at night;*" which have since been extended and improved, and republished on the Continent, in collections of his works in French and German editions, which include also his papers in the *Phil. Trans.* and others which were published in the *Journal de Physique*.

did not seem to make any alteration in the powers of the torpedo. Applying a brass chain to the back of the fish, where I had put my thumb before, I found no sensation at all in my hand, though I repeated the experiment often, and applied the chain for a space of time in which I always perceived a stroke.\* This was probably owing to the weakness of the fish in winter; or perhaps because I neglected to put my finger to its opposite side. Having insulated myself on an electrical stand, and keeping the torpedo in my hand, in the manner abovementioned, I gave not the least sign of being electrified, whether I received a stroke from the fish or not. The torpedo being suspended by a clean and dry silk ribband, it attracted no light bodies, such as pith-balls, or others, put near it. A coated bottle applied to the fish, thus suspended, did not at all become charged. When the fish gave the shock in the dark, I heard no crackling noise, nor perceived any spark. When pinched with my nails, it did not give more or fewer strokes than when not pinched. But by folding his body, or bending his right side to his left side, I felt more frequent shocks. Dr. Drummond made these experiments with me.

We dissected some of the torpedos, and found, if I remember well, 4 very large bundles of nerves, passing sideways from the head into the 2 soft bodies, called muscoli falcati, and distributed by dense ramifications through their whole substance. These nerves seem to terminate in round threads, which surround certain cylinders of a transparent gelatinous substance, which seems to constitute the material part of these singular bodies that appear to be the reservoirs of the electric power: these cylinders are parallel to each other, and have their direction from the under to the upper side of the fish. I did not observe whether these soft bodies changed in size when the torpedo gives a shock, but I suspect they do.

*II. Of Two Giants Causeways, or Groups of Prismatic Basaltine Columns, and other curious Vulcanic Concretions, in the Venetian State in Italy; with some Remarks on the Characters of these and other Similar Bodies, and on the Physical Geography of the Countries in which they are found. By John Strange, Esq. F.R.S. p. 5.*

Mr. S. first gives a topographical view of a part of the south-east side of a hill, called Monte Rosso, about 7 miles nearly south of Padua, in the Venetian State in Italy, and a mile to the west of Abano, a village well known, from the celebrated hot baths of that name, and which are situated at half a mile distance

\* Dr. Ingenhousz means, that he felt no shock, though he saw the animal, by the contortion of its body, give one to the chain. At that time he did not seem to know, that though the shock would be communicated by a rod of any metal, it could not be so by a chain, or where there was the least interruption of continuity.—Orig.

to the south of it. This view particularly represents a natural range of prismatic columns, of different shapes and sizes, placed in a direction nearly perpendicular to the horizon, and parallel to each other, much resembling that part of the famous Giant's Causeway in Ireland, called The Organs. The next is a similar representation of the west side of another basaltine hill, called Il Monte Del Diavolo, or the Devil's Hill, near San Giovanni Illarione, also in the Venetian State, and Veronese district, about 10 miles nearly north west of Vicenza. The prismatic columns appear to be ranged in an oblique position, along the side of the hill. This drawing however represents only a part of the Causeway of San Giovanni, which continues along the side of a valley, nearly in the same manner, to a considerable distance. Though the columns of both these hills are of the simple, or unjointed species, yet they differ very remarkably from each other in many respects, but principally in their forms, and the texture and quality of their parts. Those of San Giovanni commonly approach a circular form, as nearly as their angles will permit; which is also observable in the columns of the Giants Causeway, and of most other basaltine groups. On the contrary, those of Monte Rosso rather affect an oblong or oval figure. The columns of San Giovanni measure, one with the other, near a foot in diameter; nor do they vary much in their size; though this is often the case in similar groups, and is particularly observable in that of Monte Rosso, whose columns sometimes equal nearly a foot in diameter, while others scarcely exceed 3 inches: their common width is about 6 or 8 inches. They differ therefore very considerably, in size, from those of the Giants Causeway; some of which, it is well known, measure 2 feet in width. Nothing certain can be said concerning the length of the columns of San Giovanni, as they present only their tops to view; the remaining parts of them being deeply buried in the hill, and in some places entirely covered. The columns of Monte Rosso, as far as they are visible, measure only from 6 to 8 or 10 feet in height; which is also a small size, when compared with the height of those of the Giants Causeway, some of which measure near 40 feet. The columns of the Venetian groups manifest however all the varieties of prismatic forms, that are observable in those of the Giants Causeway, and other similar groups. But they are commonly either of 5, 6, or 7 sides; but the hexagonal form seems mostly to prevail, which is also remarkable in the Giants Causeway, and probably in most others. Nor is there less difference in the texture and qualities of these columns, than in their forms. Those of San Giovanni present a smooth surface, and, when broken, appear within of a dark iron grey colour, manifesting also a very solid and uniform texture; in which characters they correspond with the columns of the Giants Causeway, and those of most other basaltine groups. But the columns of Monte Rosso are very different in all these respects. For they have not only a

very rough, and sometimes knotty surface, but, when broken, show a variegated colour and unequal texture of parts. They are commonly speckled, as it were, more or less distinctly, and resemble an inferior sort of granite, of which Monte Rosso itself is formed, and which serves as a base to the range of columns in question. It is, in general, not quite so hard as the Alpine and Oriental granites, and is sometimes even friable. Linnæus justly observes, that this species of granite abounds in France; for I have lately seen large tracts of it in the neighbouring provinces of Auvergne, Velay, and Lionnois; and apprehend, that it likewise abounds in the Vivarey, Gevaudan, and Sevennes mountains; from the affinity observable in the physical geography of those countries. But it is equally common in Italy; for besides Monte Rosso, the bulk of the Euganean hills in general, of which that is a part, principally consists of it; and these hills occupy a considerable tract in the plains of Lombardy. It is also common in the Tuscan and Roman States: the mountain close to Viterbo, on the road to Rome, is entirely composed of it. The columns of Monte Rosso appear therefore of a different character from any hitherto described by mineralogists, who only mention those of a uniform colour and texture. But the great singularity here is, that such a range of prismatic columns should be found bedded, as it were, in a mass of granite, and composed nearly of the same substance; of which I never yet saw or heard any other instance. This circumstance seems therefore to render the causeway of Monte Rosso more curious and singular than the famous one in Ireland is known to be, from the regular articulation of its columns; the same phenomenon having lately been discovered at Staffa, one of the western islands of Scotland. Different groups of articulated basaltine columns have likewise been observed in the province of Auvergne in France; particularly by M. Beost de Varennes, at Blaud near Langeac; and by M. Desmarests, near le Mont d'Or. M. Sage also mentions another near St. Alcon, in the same province. The Monte Rosso group is, however, not only curious in itself, but very interesting, on account of the great light it seems to throw on the origin of granites in general.

It is remarkable, that the columns in the two different groups of Monte Rosso and San Giovanni, preserve respectively the same position, nearly parallel to each other; which is not commonly the case in other basaltine groups. For though the principal aggregate, which forms the Giants Causeway, stands in a direction perpendicular to the horizon; yet other small detached groups of columns also appear in the hill above, that affect by their position, different degrees of obliquity. Among the numerous basaltine hills of Auvergne and Velay, in France, which seem to abound in those provinces more than in any other part of Europe, and perhaps of the known globe, nothing is more common than to see the columns of the same group lying in all possible directions, as

irregularly almost, as the prisms in a mass of common crystal. Nor is this variety of position so observable in single columns, as in whole masses or ranges of them, which often present themselves in the same hill, disposed in different strata or stages, as it were, one above the other, many of which affect very different, and even opposite directions. The columns of San Giovanni seem bedded in a kind of vulcanic sand, which, in many parts of the hill, entirely covers them; these however probably rest at bottom on a base of basaltine rock of the same nature. Nothing is more common in the provinces of France just mentioned, than to see isolated basaltine hills almost exclusively composed of different layers of columns, which present themselves in stages, one above the other, often without any other stratum between them, resembling, in some measure, *si magna licet componere parvis*, a huge pile or stack of cleft wood. Though the columnar crystallization of Monte Rosso is the only one I have yet seen, or heard of, in a mass of granite, yet other groups of columns have occurred to me in other parts, that are equally of a heterogeneous substance or texture, though different from those of Monte Rosso, as well as from the common basaltes.

These systematic mineralogists, in general, assign the same common origin to most lapideous solids, which they suppose to be generated by deposition from an aqueous fluid. In whatever manner therefore the prismatic bodies in question are classed, on such a principle, no adequate idea can thence be ascertained concerning their origin, which seems manifestly different. For surely the structure, and other phenomena of these bodies, sufficiently prove them to be crystallizations or concretions of a particular kind, and generated immediately from an igneous fluid: for they are not only peculiar to vulcanic tracts of country; but differ, in every respect, from common crystals produced from an aqueous fluid. Every one knows, that the latter are formed stratum super stratum, by a slow and successive deposition and juxtaposition of parts, as hath been proved satisfactorily by Cappelér, Linnæus, and other writers on this subject. The same mode of generation is more particularly explained by Steno, in his excellent treatise, *De Solido intra Solidum Naturaliter Contento*. But this mode does not seem at all reconcileable with the basaltine crystallizations in question. For however these bodies may vary in their texture, yet none of them afford the least indication of an origin common to other crystals; but seem rather the effects of some intrinsic principle of organization, by which they appear to have been produced simultaneously, in a manner, on the consolidation of the whole mass of matter, in which they lie, and with which they constantly bear the greatest analogy, as before observed. It is further remarkable, that common crystals are parasitical bodies; whereas basaltine crystallizations, notwithstanding the peculiarities of their figures, rather seem to form integral parts of the masses.

to which they adhere ; and seem to acknowledge, with them, one common and simultaneous origin ; like the rhomboidal and other crystallizations in granites, and other similar vitrifiable compound stones. The common slow and limited principle of crystallization, seems not at all adequate to so great an effect, which seems exclusively attributable to an igneous fluid, on the general concretion of which, the organic principle may be supposed to have operated simultaneously in a large mass, and produced these bodies in the same manner as a linget of metal concretes at once in the mould. No other mode of generation seems reconcilable with the phenomena of basaltine aggregates. It seems also further evident from the phenomena, that prismatic basaltine crystallizations, and other regularly figured vulcanic groups, have been generated locally, and not in the midst of those violent convulsions of Nature which are commonly assigned for the origin of vulcanic mountains in general. That the principle of organization, whatever it be, operates locally in the formation of these bodies, appears sufficiently evident from the regular disposition and other particular characters of their groups. For notwithstanding the various directions of the columns, and masses composed of them, in the different groups, yet in other respects the greatest regularity of disposition is commonly observable. They form strata, which are uniformly organized, disposed in particular directions, and often constant in the same to a great extent. These strata not only manifest a parallelism between their regularly figured parts, but in their whole aggregates ; which often form extensive horizontal beds, and of an equal thickness throughout. This parallelism is also equally remarkable in groups that are composed of many strata ; as I have particularly observed in those of Murat, and the Castle Hill of Achon, in Upper Auvergne ; in which the columnar strata are not only parallel in themselves, but preserve in their position, a parallelism with the other strata of the respective groups, which lie in regular stages, one above the other : and since these groups commonly form, in a manner, integral parts of the masses, or mountains in which they are found, and these manifest also some affinity in their structure ; it seems most reasonable to assign to both one common origin.

The Euganean hills form an irregular group in the plain of Lombardy, about 7 miles nearly south by west from Padua, and extend from north to south as far as Este. The most considerable part of them composes an irregular sort of chain, which extends in the above direction ; while other parts are severally detached, and form isolated mountains about the skirts of this chain, particularly on the north-east side, towards Abano. The outer skirt of the entire group may measure from 30 to 40 English miles. The external characters of this group exactly correspond with the forms commonly ascribed by naturalists to vulcanic mountains in general ; since the points of the chain before mentioned, as well as the isolated members of it, are of various conical, orbicular, and elliptical shapes.

As this group, therefore, rests on a perfect plain, it makes a very singular appearance. The volcanic hills immediately round Isenchaux in Velay affect also the same forms; but as they are mixed with other hills of a different form, and the country about them is broken and irregular, they do not produce so singular an effect as the Euganean hills, which suddenly rise from a perfect level. I am informed, that there is a similar, though smaller group of isolated volcanic hills in a plain of Dalmatia, near Cossovo; and another group of hills, nearly of the same forms, in the county of Down, in Ireland, and called the Mourn hills; which, like those near Padua, consist mostly of granite and lava. The Euganean hills have, moreover, a superficial and partial covering of slaty and calcareous strata, of posterior origin, and that manifest no marks of having suffered by fire. Such strata slightly cap mount Venda, which is the highest among these hills; though of no very considerable elevation, measuring only about 252 French toises above the Venetian Lagunes, according to Abbé Toaldo, professor of astronomy at Padua. From the lava and granite mixed together in the Euganean hills, they bear an affinity with those of Auvergne and Velay; but differ from them by the superincumbent unburnt strata of lime-stone.

*III. An Inquiry, to show what was the Ancient English Weight and Measure according to the Laws or Statutes, prior to the Reign of Henry the Seventh. By Henry Norris, Esq. p. 48.*

William the Conqueror, by his charter, confirmed to the English all their ancient laws, with such additions or alterations as he made to their advantage. The 57th clause of that charter is, "De mensuris et ponderibus. Et quod habeant per universum regnum, mensuras fidelissimas et signatas, et pondera fidelissima et signata sicut boni prædecessores statuerunt." From this clause it seems clear, that king William ordained sealed standards, both of weights and measures, to be made, such as his predecessor king Edward had ordained. Neither weights nor measures are here described particularly; but the subsequent statutes define them more plainly. And the *Chronicon Pretiosum* tell us, that from historians it appears the Conqueror determined what the weight of the sterling penny, or penny-weight should be, to weigh 32 grains dry wheat. Consequently the standard penny-weight was made equal to the weight of 32 grains of wheat. Succeeding kings confirmed William's charter; and even the great charter granted by king John is only to explain and restore the ancient laws, which had been infringed. The statutes of 51st of Henry III, and 31st of Edward I, explain the ancient weights and measures; that is to say, the English penny called a sterling, round without clipping, was to weight 32 grains dry wheat, taken from midst of the ear, and 20 of those penny-weights were to make an ounce, and 12 ounces a pound; and 8 of those pounds were to be a gallon

of wine, and 8 of those gallons to make a London bushel, which is the 8th part of a quarter. The definition of the penny-weight, in these statutes, agrees with the determination of William the Conqueror, and shows that the legal weight continued the same. What the weight of that pound was, so raised from a penny-weight, equal to the weight of 32 grains of wheat, we may clearly learn from that declaration in the 18th of Henry VIII, when he abolished that old pound, and established the Troy weight; which says, that the Troy pound exceedeth the old Tower pound by  $\frac{2}{3}$  of the ounce. As the Troy pound established by Henry VIII, is the same as is now in use, consisting of 5760 Troy grains, and 480 grains to the ounce, and 12 ounces to the pound; so 360 grains is  $\frac{1}{16}$  of the ounce, which, deducted from 5760, leaves 5400 Troy grains, equal to the weight of that old Saxon pound which he abolished. But to trace out experimentally the weight of that penny-weight, raised from 32 grains of wheat, I got a small sample of dry wheat of last year 1773 (the weight of that year but ordinary); and, from a little handful of it, I told out just 96 round plump grains, dividing them into parcels of 32 grains each, and all 3 weighed exactly  $22\frac{1}{4}$  Troy grains; consequently, 240 such penny-weights, which the old pound consisted of, were equal only to 5400 of our present Troy grains, conformable to the declaration of Henry VIII. Thus the weight of that old pound is clearly ascertained to be lighter than the present Troy pound by  $\frac{1}{4}$  of an ounce; and it clearly shows that they were 2 different weights.

By those statutes of Henry III, and Edward I, it is said, that 8 pounds were to make a wine gallon, and 8 of those gallons to be a bushel, and 8 bushels a quarter; consequently the wine and corn gallon were one and the same measure. The statute of the 12th of Henry VII says, the gallon measure was to be 8 pounds of wheat, which ascertains what was to be understood by former statutes, and is consonant to reason, to fix the measure of wheat by its own weight, not by that of wine, as wheat was an article of greater importance to the community to ascertain its measure, than wine; and a gallon measure to contain 8 pounds of wheat, must be  $\frac{1}{4}$  part larger in cubical contents than a measure to contain 8 pounds of wine. As it appears by the charter of William the Conqueror, that there were sealed standards made of weights and measures, we cannot doubt, but they were preserved and kept in the king's exchequer, for legal standards; and as several statutes direct their being made of metal, they were permanent and certain, by which to make more: which Henry VII expressly tells us he practised, by making new according to the old: so that there could be no need to recur to 32 grains of wheat, much less to 7680, every time new standards were to be made, unless we suppose our ancestors defective in common sense. Whenever, by new statutes, fresh standards were directed to be made, we may observe that the assize of weight and measure continued uniformly fixed and de-



scribed to be one and the same, to show there was no alteration made or intended. And thus, by the laws of assize, from William the Conqueror to the reign of Henry VII, the legal pound weight continued a pound of 12 ounces, raised from 32 grains of wheat, and the legal gallon measure invariably to contain 8 of those pounds of wheat, 8 gallons to make a bushel, and 8 bushels a quarter; the bushel therefore contained 64 of those pounds of wheat, and the quarter 512 pounds.

These were the legal weights and measures for common use, during that period. The first alteration really made therein, was in the 12th year of Henry VII. That the laws of assize were often infringed, is very evident from the frequent complaints, mentioned in Cotton's Abridgment of the Tower Records, against the king's purveyors; particularly in the 14th of Edward III, for remedy against outrageous takings of purveyors; and in the 45th of Edward III, that the king should be served by common measure; and in the 3d of Henry 5, that the king's purveyors do take 8 bushels of corn only, to the quarter struck. The general answers to which were, that the statutes should be observed. It appears also, that others infringed the laws of assize. For the statute of 27th of Edward III says, some merchants bought Avoirdupois merchandises by one weight, and sold by another; which plainly implies, they bought by some weight heavier than the legal, and sold by the legal weight which was lighter. The statute therefore, to enforce observance of the laws of assize, only wills and establishes, that there be one weight, one measure, and one yard, through all the land. This can be understood to mean no other than the legal assize, which preceding statutes had enacted. And further, in the reign of Henry VI, we see that buyers of corn, bought by a vessel, called a fat, of 9 bushels, which contained 72 gallons; and like those merchants before mentioned in the statute of Edward III, we may presume they sold by another measure, the legal quarter of 8 bushels, containing but 64 gallons: for the statute of 9th Henry VI forbids the buying by that vessel, called a fat. The prohibition implies the illegality of the vessel and its use, and implies also the enforcement of the laws of assize. Taking therefore all the several statutes together, in one connected view, those that fix the laws of assize, with those to reform abuses committed against them, we are led to conclude, that those laws of assize continued uniformly one and the same, till Henry VII altered them. Having thus shown by those laws, that the old pound weight was a Saxon pound of 12 ounces, raised from 32 grains of wheat, and was equal only to 5400 of our present Troy grains; and that the measure of capacity was a gallon, to contain 8 of those pounds of wheat, and 8 of those gallons made a bushel: I shall now endeavour, by help of figures, to demonstrate what was the cubical contents both of the gallon and bushel measures.

We know that the present Troy pound consists of 5760 Troy grains, and that

7000 of those Troy grains are equal to the present Avoirdupois pound of 16 ounces, and that 5400 of those Troy grains are equal to the old Saxon pound of 12 ounces; consequently, the old Saxon pound was  $\frac{3\frac{1}{4}}{4}$  of the present Troy pound, and the old Saxon pound was  $\frac{7}{8}$  of the present Avoirdupois pound. We know that modern experiment has proved the weight of 1728 cubic inches of wheat, common sort, to be  $47\frac{1}{4}$  pounds Avoirdupois; and of a better sort, to weigh from  $48\frac{1}{4}$  to  $48\frac{1}{4}$  pounds Avoirdupois; the difference in their weight is not very great; however I shall take the lowest weight to compute by, the  $47\frac{1}{4}$  pounds Avoirdupois, which, in Saxon weight, is  $61\frac{3}{4}$  pounds Saxon. And then say, as  $61\frac{3}{4}$  pounds Saxon : 8 pounds Saxon :: 1728 cubic inches :  $224\frac{1}{4}$  cubic inches, for the contents of the old Saxon gallon for wine and wheat. But as the old standard wine gallon kept at Guildhall, and found there in 1688, proves to be 224 cubic inches contents, there is reason to conclude it to be of the same standard assize, as was the ancient Saxon gallon for wine and wheat: for, as 1728 cubic inches : 224 cubic inches :  $61\frac{3}{4}$  pounds Saxon :  $7\frac{3}{4}$  pounds Saxon, which is about  $4\frac{1}{4}$  penny-weights short of the 8 pounds, mentioned in the statutes for the gallon to contain, and is such a small difference, as may arise in different years, in the weight of such a quantity of wheat. The very near agreement of these computations, gives us sufficient reason to conclude, that the old standard wine gallon, of 224 cubic inches contents, found at Guildhall in 1688, was of the same standard assize, as was the ancient gallon measure ordained to hold 8 Saxon pounds of wheat; and of course then the bushel measure must have been 1792 cubic inches contents, which will appear to hold nearly 64 Saxon pounds of wheat, as by those old statutes it ought to do. For, as 1728 cubic inches : 1792 cubic inches ::  $61\frac{3}{4}$  pounds Saxon :  $63\frac{1}{4}$  pounds Saxon, which is only about an ounce and three quarters short of 64 pounds; and in so large a quantity of wheat, is a trifling difference, naturally arising in weight of wheat of different years. These demonstrations, by figures, sufficiently prove what the cubical contents of those ancient English measures must have been, according to the old statutes of assize, viz.

The gallon measure, 224 cubic inches contents, to hold 8 pounds Saxon.

The bushel. . . . . 1792 ditto . . . . . 64 ditto.

And as 8 bushels made a quarter, the quarter contained 512 Saxon pounds of wheat. These were the ancient legal measures, according to the old laws of assize.

It now remains to mention the particular statute of the 12th of Henry VII, under which an alteration was brought about in those ancient weights and measures, without seeming to intend it; as the statute itself differs not in substance from the other old laws of assize, except calling the pound by a new name, Troy. But previously it may be observed, that very probably the unsettled state

of the kingdom for many years preceding, might pave a way to that alteration. There had been several contests about the crown, between the two houses of York and Lancaster, till Henry VII by conquest mounted the throne; and in such times of public disturbance, the laws of assize were more likely to be infringed than well kept. For, after Henry VII was well settled on his throne, we find complaint was made in the 11th year of his reign, that the laws of assize had not been observed and kept. On which he made fresh standards of weights and measures, and sent them to the several shires and towns in the kingdom. But in the very next year, the 12th of his reign, there came out that particular statute, under which the weights and measures were altered; reciting that the king, in the former year, had made weights and measures of brass, according to the old standards remaining in his treasury, which weights and measures are said, on a more diligent examination, to have been approved defective. It is not said, whether they were the old standard weights and measures, or the new ones, made in the former year, that had been approved defective; nor how much they were so; all this is left to conjecture. Therefore we may with great probability conjecture, that they were not defective in respect to their old original standard; but only in respect to the heavier new Troy pound, intended to be then introduced. And what warrants such conjecture is, the express declaration of his son Henry VIII, when he abolished the old pound, in the 18th of his reign, and established the Troy; for he then declares, that the Troy pound exceeds the old pound by  $\frac{1}{4}$  of an ounce. Hence then, there can be no doubt, but Henry VII altered the old English weight, and introduced a heavier Troy pound, that exceeded the old one by  $\frac{1}{4}$  of an ounce; and though none of his standard weights have come down to us, yet his brass bushel measure, with his name on it, was found in the exchequer in 1688, and proves to be 2145 cubic inches contents; from which we may form conclusions, both on his weights and measures, sufficient to convince us that he altered both. That his bushel was a measure of 9 gallons instead of 8, and that his Troy pound was  $\frac{1}{16}$  part heavier than the old English pound, which was raised from 32 grains of wheat. Experiment has proved, that a measure of 1728 cubic inches of wheat, will weigh from  $47\frac{1}{4}$  to about  $48\frac{1}{4}$  pound Avoirdupois; but suppose it be only  $47\frac{1}{4}$  pounds Avoirdupois, that, in Troy weight, will be  $58\frac{1}{8}$  pounds Troy. Hence we may easily find the weight of wheat that 2145 cubic inches will contain. For, as 1728 cubic inches : 2145 cubic inches ::  $58\frac{1}{8}$  pounds Troy : 72 pounds Troy, the weight of wheat that Henry VIII's bushel would contain. And dividing the 72 by 8, the number of pounds limited by the statute to a gallon, it proves Henry VIII's bushel was a measure of 9 gallons instead of 8; and as 8 bushels made a quarter, then the quarter contained 72 gallons; which seems to correspond with the number of gallons contained in the vessel, called a fat, the use of which was

prohibited by statute in Henry viii's time, about 60 years before Henry vii, as before remarked. If we divide the 2145 cubic inches contents of the bushel, by 9, the number of gallons it contained, it shows the gallon measure to be  $238\frac{1}{8}$  cubic inches contents, which is  $\frac{1}{8}$  part larger than the old Saxon gallon of 224 cubic inches, just in the proportion as the Troy pound is  $\frac{1}{8}$  part heavier than the old Saxon pound. The statute limits the gallon to hold 8 pounds Troy of wheat; and so we find the gallon of  $238\frac{1}{8}$  cubic inches will do; for as 2145 cubic inches :  $238\frac{1}{8}$  cubic inches :: 72 pounds Troy : 8 pounds Troy. But if it be said, that the statute limits the bushel to 8 gallons, not 9, then the gallon measure must have been  $268\frac{1}{8}$  cubic inches contents, and would hold 9 pounds Troy of wheat, though the statute says it was to hold only 8 pounds Troy. Take it either way, it shows that the bushel was not made according to the statute; it held 72 pounds instead of 64 pounds. And on the whole it clearly proves, that Henry vii altered both the weights and the measures; that he introduced the Troy pound, which was heavier by  $\frac{1}{8}$  of an ounce than the Saxon or old English pound; and that his bushel measure was about  $\frac{1}{8}$  part larger than the ancient Saxon or old English bushel measure. The first statute that directs the use of the Avoirdupois weight, is that of the 24th of Henry viii; which plainly implies it was no legal weight, till that statute gave it a legal sanction, and the particular use to which the said weight is there directed, is simply for weighing butchers meat in the market. And it is note-worthy, that in all the old statutes of assize prior to Henry vii, the legal gallon measure of capacity is founded on 8 pounds, raised from the weight of 32 grains of wheat, and by that statute of 12th Henry vii, the gallon is to contain 8 pounds Troy: therefore these 2 sorts of weight were the only ones established as legal by the statutes; and both are a lighter weight than Avoirdupois. How, or when, the Avoirdupois weight came first into private use, is not clearly known to us; but this seems clear, that no statute before the 24th Henry viii has given it any legal sanction.

IV. *Of an Apparatus for Impregnating Water with Fixed Air; and of the Manner of Conducting that Process.* By John Mervin Nooth, M.D., F.R.S. p. 59.

The possibility, says Dr. N., of impregnating water with fixed air was no sooner ascertained, by experiment, than various methods were contrived to effect the impregnation. Dr. Priestley, however, is the only one that has published any description of an apparatus, calculated entirely for this purpose. This apparatus was communicated to the public, with the view of promoting the discovery of the medical effects of fixed air united with water; and, in consequence of this communication, some very successful attempts have been made in the cure of diseases. The experiments however have not been so numerous as one could

have wished; perhaps the difficulty in conducting the process, in the manner proposed, has been, in some measure, the reason why so few experiments, on this subject, have been made public. For though, in the hands of Dr. P., the apparatus was sufficiently convenient, it must be confessed, that the conduct of the process required more address, than generally falls to the share of those that are unaccustomed to such experiments. Independent too of the inconveniencies attending the process, there was another objection to the apparatus, which, with most people, might have considerable weight. The bladder, which formed part of it, was thought to render the water offensive; and when the solvent power of fixed air is considered, it will not appear improbable, that the water would be always more or less tainted by the bladder. In some trials which Dr. N. made with Dr. Priestley's apparatus, it always happened, that the water acquired a urinous flavour; and this taste in the water was, in general, so predominant, that it could not be swallowed, without some degree of reluctance. The difficulty therefore in the conduct of the process, and the offensiveness of part of the apparatus, made some less exceptionable method of producing the impregnation desirable. This Dr. N. variously attempted, keeping convenience and cleanliness constantly in view; and he flattered himself, that he had at last contrived an apparatus that would perfectly answer the intended purpose. Twelve months had elapsed since this contrivance had been in constant use; and to that time there was no reason to wish for the least alteration. Presuming therefore on the possibility of its becoming, when known, extensively useful, and convinced of the favourable reception which every attempt of this nature meets with from the R. S., he begged leave to communicate a description of the apparatus that he had invented, and of the manner of conducting the process.

The apparatus is of glass, and consists of 3 vessels as A, B, C, fig. 1, 2, 3, pl. 12. The glasses are accurately fitted to each other, and at the joints are impervious both to air and water. The glass A is designed for the effervescing substances. The vessel B is to contain the water to be impregnated with air. In the lower part of the glass B is placed an ivory valve, surrounded with cork, as in fig. 4. The cork a is fitted to the bottom of the glass B, and has through it a hole, to receive the part b of the ivory valve. On the broader part of this piece b, is placed a moveable piece c. The surfaces of these pieces are so accurately ground, that, when applied to each other, no fluid whatever can pass between them. The moveable part c is secured on the part b by the cover d, which is so constructed, as to allow the piece c some motion, and this cover has likewise holes to give passage to the air that shall raise the moveable piece c. The glass c serves 2 purposes; it confines the air on the surface of the water in B, and at the same time prevents all danger of explosion by allowing the water to give place to the ascending air.

*The Process.*—As chalk and oil of vitriol are capable of producing the desired effervescence, and are the most eligible on account of their cheapness, he has, in describing the process, mentioned only these 2 ingredients. Various other substances may however be employed for the same purpose; but none perhaps are so unexceptionable as those named. In the other acids a proper degree of fixity is wanting, during the effervescence; the nitrous and marine have so much volatility, that there is always a risk of some of the acid fumes passing the valve, and thus rendering the water acid, which it was intended to impregnate only with fixed air. To begin the process, it is necessary to fill the vessel A up to the dotted lines, with diluted oil of vitriol. By confining the height of the surface of the effervescing mixture to the dotted lines in the glass A, none of the acid will be driven through the valve, during the intumescence that attends the escape of the fixed air. The glass B is to be totally filled with water, and the vessel C is to be put on it. Some powdered chalk is then to be thrown into the glass A, and the vessels are to be immediately placed as in fig. 5, except that the stopper belonging to C is to be left out. When the acid in the lowermost vessel acts on the chalk, the extricated air passes the valve in the middle glass; and as the construction of this valve allows the fixed air from the effervescing substances to pass, but denies a passage to the water in a contrary direction, the separated air ascends to the upper part of the middle glass, and at the same time a portion of water, equal in bulk to the intruding air, passes up the bent tube into the uppermost vessel. As the effervescence goes on, the fixed air continues to accumulate in the middle vessel, and the uppermost one to be filled with the water that has given place to the air. The quantity of chalk to be thrown into the acid at one time, must be determined by the capacity of the uppermost vessel. Should more air be extricated than is sufficient, in the conduct of the process, to fill that vessel, the water will run over the top of it, and will continue to run as long as any air ascends in the middle vessel, or till the surface of the water is below the extremity of the bent tube. Both these accidents are to be carefully avoided; as in one case the whole would be wet and disagreeable; and in the other, a quantity of fixed air would be unnecessarily lost. Half a drachm of chalk will, in general, produce air enough to fill the uppermost vessel with water; and it must be remembered, that the chalk employed to produce the effervescence, should be finely powdered, as a selenetic crust will otherwise form around it, and thus prevent the action of the acid on the interior part. To keep the neck of the glass clean, through which the chalk is put, it will be necessary to include the chalk loosely in paper; and this circumstance is by no means to be neglected, as the accurate junction of the glasses depends on it, and consequently the whole of the process. When the uppermost vessel is filled with water, and there is therefore a considerable quan-

tity of fixed air in the middle one, these two vessels are to be separated from the lowermost, and the air and water are to be agitated together, to promote their union. If, during the agitation, a stopper be put into the uppermost glass, the descent of the water in it will not show the absorption of the fixed air by the water, as the external atmospherical air will enter below, at the valve, to fill the space which the absorbed fixed air would otherwise leave void. But, on the contrary, if the uppermost vessel be open, during the agitation, the pressure of the atmosphere on the surface of the water in that vessel, will force the water down into the middle one, as fast as the absorption of the fixed air below will allow it room. This latter method may be pursued, when a person wishes to know the quantity of fixed air that the water can absorb; but in common use, it will be better to stop the uppermost vessel, as the air and water may be then more forcibly agitated without inconvenience, and of course the impregnation more expeditiously effected. During the effervescence, the uppermost glass is to remain open, and it is only to be stopped when the agitation is performed. It is not to be expected, that the impregnation will be considerable at first; it will indeed be necessary to repeat the process, with the same water, 4 or 5 times, before it will be highly impregnated. After an agitation therefore, when a stronger impregnation is wished for, the uppermost vessel is to be opened, and raised from the middle one, to allow the water to descend, that was before driven up. When the middle glass is again full, a fresh quantity of chalk is to be put in the lowermost vessel, and the agitation to be repeated, as soon as the effervescence ceases. It is seldom necessary to repeat the process more than 4 times, to produce a very strong impregnation; but should it be thought proper to have the water as highly saturated with fixed air as it admits of, nothing more than a repetition of the same process is requisite. In this account of the apparatus, he had purposely confined himself to the method of uniting fixed air with water; but it is to be observed, that many curious experiments may be made with it, both in chemistry and pharmacy. By its assistance, Dr. N. had been enabled to imitate very perfectly, the common mineral waters, and to make aqueous solutions of substances that were before deemed insoluble in water. These circumstances however he had reserved for a future paper, which he should have the honour to present to the society, as he had not then been able to arrange the several facts, which this apparatus had made him acquainted with, in the manner he could wish.

P. 8. Since the foregoing paper was read, Dr. N. had contrived a glass valve, which seems preferable in some respects to the ivory one. The following is a description of it. It consists of 3 pieces, as in fig. 7. The superior and inferior pieces are perforated, but the middle one is without perforation, having only its upper part convex, and its under part plane. In fig. 8 is a perpendicular

section of the 3 pieces composing the valve, at the distance at which they ought to be placed, with respect to each other, in the tabular part of the vessel B. This vessel having the glass valve in it, and filled with water, is to be put on the glass A containing substances in the act of effervescence. In that case, the extricated air will ascend through the perforations in the superior and inferior pieces, the middle one proving no obstacle to the air, having sufficient room to yield to the current of air rushing upwards; but when the air ceases to ascend, and the pressure of the water above takes place, the middle piece will prevent the water from descending, its plane surface being then applied to the plane surface of the piece below it. Thus this glass valve will answer in every case where the ivory one can be employed; and for a variety of purposes it will undoubtedly prove preferable, particularly when corrosive substances are subjected to experiment.

*V. Of a Musical Instrument, brought by Captain Fourneaux from the Isle of Amsterdam in the South Seas, to London, in 1774, and given to the R. S. By Joshua Steele, Esq. p. 67.*

This instrument consisted of a system of 9 musical pipes, of various lengths, and connected together in a parallel position. The manner of blowing them, in making the experiments, was the same as people use to whistle in the pipe hole of a drawer key. The upper series of tones, which are exact 5ths to the lower, are easiest produced by an unexperienced person; and the lowest series, which we shall call fundamentals, with somewhat more address and a weaker blast. Besides the abovementioned tones, if the velocity of the breath be increased a little, the first 5 pipes will give octaves to the fundamentals, and if further increased, sharp 3ds, or tierces, above these octaves. In the pipes 6, 7, 8, 9, Mr. S. could neither make the octaves to the fundamentals, nor the sharp tierces; but in their stead, the minor, or flat 3d, above the octave came, when the breath was urged beyond the degree requisite to produce the 5th. This minor 3d, is an accident out of the natural order of tones produced from simple tubes, which he does not pretend to account for. Mr. S. then adds the notes of the several tones which he produced from each pipe.

*VI. Remarks on a Larger System of Reed Pipes from the Isle of Amsterdam, with some Observations on the Nose Flute of Otaheite. By Joshua Steele, Esq. p. 72.*

The nose-flute of Otaheite, gives only 4 sounds, with the first degree of breath, which are, in an ascending series, by a semitone, a tone and a semitone. If urged with a stronger breath, it will give octaves above these; but it then becomes ill in tune: and it seems, the natives of Otaheite use no more than those first 4



sounds. Notwithstanding the small extent of this series, yet, by the aid of varying the measure, it is capable of several different melodies, though the general cast of them will be melancholy.

The specific difference between this system of pipes, and the smaller, described before, will be understood from the following observations. It consists of 10 pipes, joined together in the same manner as those of the smaller system. The first 9 pipes exhibit to the eye the same figure as the system before described; and the 10th pipe is a little longer than N<sup>o</sup> 4. For in this larger system, N<sup>o</sup> 8 is 13 inches long; N<sup>o</sup> 4 13 $\frac{1}{4}$ , nearly; and N<sup>o</sup> 10 is 14 inches. The sounds which each pipe exhibits easily, are marked in minims. As the upper minims are 6ths to those next under them, it follows, from the law of harmonic sounds, that the lower minims are 5th to the fundamental sounds of these pipes, which are written in quavers, to show that they are very difficult to be produced. The upper minims of N<sup>o</sup> 1, 2, 3, 4, 5, and also of 10, are sharp 3ds, or rather, major 10ths, to the fundamental sound of each pipe. And the upper minims of N<sup>o</sup> 6, 7, 8, 9, are nearly minor tenths to their fundamentals; which circumstance seems to agree with what was remarked in the smaller system, as an extraordinary property, touching the minor 3ds. But Mr. S. will not yet assert, that this property is altogether natural, because he found some of the latter pipes were partly obstructed by accidental rubbish, which was drawn out with difficulty: so that he pretends not to decide, whether the cause of their being, not quite, in the same proportion of tune, as he found in the first system, arises from some casual injury, or from original intention, or original inaccuracy. The interval between N<sup>o</sup> 1 and 2 in these pipes, is only of 2 semitones; whereas that between the N<sup>o</sup> 1 and 2 of the former system, was of 3 semitones. The series N<sup>o</sup> 2, 3, 4, 5, and the series N<sup>o</sup> 6, 7, 8, 9, have similar intervals in both systems. Therefore he imagines these to have been the original extent of the whole modulating series, like the double tetrachord of the Greeks, and that the N<sup>o</sup> 1 and N<sup>o</sup> 10 are additional at pleasure; as, in the smaller system, the interval between N<sup>o</sup> 1 and 2 was a semitone greater than that between N<sup>o</sup> 1 and 2 in the larger system; and N<sup>o</sup> 10 in the smaller system was totally omitted, though he had seen 2 others which had it. The sounds in this larger system are 7 tones lower than those of the smaller, which corresponds with the difference of their dimensions; the pipe N<sup>o</sup> 4 in this system measuring nearly 13 $\frac{1}{4}$  inches in length, with diameter seemingly proportional; whereas the N<sup>o</sup> 4 in the smaller system measured only 7 $\frac{1}{4}$  inches. By increasing the velocity of the blast, these pipes gave sounds still higher, which were 4ths above the upper minims, or octave and 6ths above the fundamentals; and with a little more force, tritones, or sharp 4ths, above the upper minims, which were octave and flat 7ths above the fundamentals.

*VII. Description of a New Dipping-Needle. By Mr. J. Lorimer, of Pensacola.*  
p. 79.

Whenever any one meets with a terrella, or spherical loadstone, the first thing he does is to find out its poles; and having once discovered them, he knows immediately how any small bit of needle will be affected, when placed on any part of its surface. The poles are most readily discovered by trying where the filings of iron, or a small bit of needle, will stand erect on the terrella; and this is generally found to be on 2 points diametrically opposite to each other. But the magnetic poles of the earth seem to be situated obliquely to one another (see the Berlin Memoirs, 1757); but where they are actually situated, is hitherto unknown; whether they are on land or water. Yet be these things as they may, it appears evident, that accurate observations, made as near to these magnetic poles as possible, with a good dipping-needle, are the surest way to complete the magnetic theory of this globe, analogous to the method we pursue in examining the terrella. But as all the dipping-needles appeared to be very ill calculated, for the sea service at least, Mr. L. contrived one on a different plan in 1764, and had it executed by Mr. Sisson. He called it a universal magnetic needle, or observation compass; because he could by it take the dip and amplitude, and even the azimuth, with only one assistant, to take the altitude. The needle was of the same shape and size nearly as those used for the compasses of the royal navy, and played vertically on its own axis, which had 2 conical points, slightly supported in 2 corresponding hemispherical sockets, inserted into the opposite sides of a small upright brass parallelogram, about  $1\frac{1}{4}$  inch broad, and 6 inches high. Into this parallelogram is fixed, at right angles, a slender brass circle, about 6 inches diameter, silvered and graduated to every half degree, on which the needle shows the dip; and this, for the sake of distinction, he calls the circle of magnetic inclination. This brass parallelogram, and consequently the circle of inclination, also turns horizontally on 2 other pivots, the one above and the other below, with corresponding sockets in the parallelogram. These pivots are fixed in a vertical brass circle, of the breadth and thickness of  $\frac{1}{16}$  of an inch, and of such a diameter, as to allow the circle of inclination and the parallelogram to move freely round within it. This 2d he calls the general meridian. It is not graduated, but has a small brass weight fixed to the lower part of it, to keep it upright; and the circle itself is screwed, at right angles, into another circle, of equal internal diameter, of the same thickness, and twice the breadth, which is silvered and graduated on the upper side to every half degree. It represents the horizon, as it swings freely on gimbols, and is always nearly parallel to it. The whole is contained in a neat mahogany box, of an octagon figure, with a glass plate at top and one on each side, for about  $\frac{1}{3}$  down. That part of the frame which contains the

glass lifts off occasionally. The whole box turns round on a strong brass centre, fixed in a double plate of mahogany, glewed together cross-ways, to prevent its warping or splitting; and this again is supported by 3 brass feet, such as are used for the cases of table knives, frosted that they may not easily slip, if the vessel should have any considerable motion. It has another square deal box to lock it up in, to preserve the glass, &c. when it is not wanted for use.

The use of this instrument is very plain, as the inclination or dip is at any time apparent from inspection only, and also the variation, when the frame is turned round till the great vertical circle lies exactly in the plane of the true meridian: for the circle of inclination being always in the needle's vertical plane, its edge will evidently point out on the horizon, the variation E. or W. But at sea, when there is not too much motion, the frame is turned round, till the vertical circle be in the plane of the sun's rays; that is, till the shadow of the one side of it just covers the other, and the edge of the circle of inclination will then give the magnetic amplitude, when the sun is rising or setting; but the azimuth at all other times of the day; and the true amplitude or azimuth being found in the usual way, the difference is the variation. When the motion is considerable, observe the extremes of the vibration, and take the mean for the magnetic amplitude or azimuth. When the sun does not shine so bright as to give a shadow, set the brass circle in a line with his body, if he be at all visible by the eye. The principal advantage at first aimed at in this compass, was to contrive a dipping-needle, which should be sufficient for making observations at sea. As those needles, to be of use, must be placed, by some means or other, in such a manner, as that all their vibrations shall be made in the true magnetic meridian, north and south, otherwise they are good for nothing. For if one of them be placed at right angles, across the magnetic line, it will stand perpendicularly up and down in any part of the world; the least dip therefore is always in this magnetic line. But the only method of setting a dipping-needle at sea, was to place it in a line with the common compass needle; and this must be very inaccurate, if they be at any considerable distance from each other; or if they be near, the 2 needles would influence each other, and neither of them could be true; nay, supposing them for once to be properly placed in this line, the least motion of the ship throws them out again. But this instrument has a constant power in itself, not only of setting itself in the proper position, but also of keeping itself so; or of restoring itself to the same situation, if at any time it has lost it; and it is curious to see how, by its double motion, it counteracts, as it were, the rolling motion of the vessel.

*VIII. Bill of Mortality, for Chester for the Year 1773. By J. Haygarth, M. D., F. R. S., p. 85.*

That Chester is healthy to a very remarkable degree, is still more clearly evinced from the register of this year, than in that of the last. In 1772, one half of the inhabitants appeared to arrive at 20 years of age; a fact which seemed very surprizing when compared with the proportional mortality in other towns, both of a larger and less size. But, according to this year's register, one half have lived to be 36 years old. In 1772, 1 in 15 $\frac{1}{2}$  had lived to above 80, and this year 1 in 13. These are very uncommon instances of longevity for so large a proportion of the inhabitants. The inhabitants of St. Michael's parish were numbered to be 618, of whom this year 10 only have died; that is, a less proportion than 1 in 61. If the inhabitants of the whole city were numbered with the same accuracy as those of St. Michael's, many important conclusions, both medical and political, might with certainty be deduced from the bill of mortality. The register of burials in the 9 parishes are kept separate; hence, by comparing the number of inhabitants in each parish with the burials in each, for a period of years, we may, on the most evident foundation, discern which part of the town is most healthy. In a political view, such an account would furnish the best means of demonstrating the accuracy of a table of the probabilities of life, formed from the register, and supply unerring data for calculating annuities, the value of reversionary payments, and assurances on lives. Such an old town as Chester, where the number of inhabitants has for many years suffered little variation, and where the births and burials are nearly equal, is peculiarly well fitted to furnish this important information. The registers confirm the observation, that women live longer than men. Of those who have lived to above 80, only 10 are males, and 17 females; the number of widowers this year is 17, of widows 44. The table of diseases of different ages confirms in general the observations of last year. It is evident that no epidemic visited this place in 1773; not one died of the measles, or miliary fever, and the 10 who sunk under the chincough had probably lingered under the disease since the former year, towards the end of which it ceased to be epidemic. Only one died of the natural small-pox; 12 were inoculated in Chester, during this year, and all recovered.

*XX. Experiments on a New Colouring Substance from the Island of Amsterdam, in the South Sea. Made by Mr. Peter Woulfe, F. R. S. p. 91.*

This substance is of a light bright orange colour; has a peculiar, though not a strong smell; and, when handled, gives a yellow stain to the skin, which does not readily wash out with soap and water. Put on a red hot iron, it smokes, melts, and catches fire, leaving a caput mortuum. When boiled with water, it gives the liquor only a slight yellow tinge, which is but little heightened by the addition of a fixed alkali; therefore the colouring part of this substance is

insoluble in water. Oil of vitriol put to it becomes of a red orange colour ; but, when the acid is drained off, the residuum appears purple. Annotto, treated in the same manner, gives a blue colour. Spirit of wine, æther, fixed and volatile alkalies, as also soap, dissolve the colouring part of this substance. To determine the quantity of colouring matter which it contains, 2 drs. were digested in a matrass, with 4 oz. of rectified spirit of wine; the solution, being filtered, assumed a rich deep yellow colour, like a strong solution of saffron or gamboge with the same spirit; what remained in the filter was digested a 2d time, with 4 oz. of fresh spirit of wine, and the liquor filtered; this solution was much weaker than the first. The undissolved part remaining in the filter after this 2d solution was digested, a 3d time, with 4 oz. of fresh spirit; but the solution was now quite weak, and of a very pale yellow colour. The residuum being now deprived of its colouring portion, was slowly dried, when it appeared of a very pale yellow colour, felt as soft as starch between the fingers, and weighed 42 grs.; so that  $\frac{2}{3}$  nearly of this colouring substance are soluble in spirit of wine; the undissolved part is not soluble in water, acids, or alkalies. Put on a red hot iron, it smokes and catches fire without melting, leaving a caput mortuum, and gives a smell similar to that arising from common vegetable matter. The first solution in spirit of wine, after standing 24 hours, deposits some of its colour in the form of minute spiculine crystals, of an orange colour. The 2d and 3d solutions let fall none of their colour. The 1st solution, dropped on paper, tinges it of a bright orange colour, the 2d gives a lively yellow colour, and the 3d a pale yellow. The 1st solution, sufficiently diluted with spirit of wine, makes a bright yellow stain on paper, no way inclining to an orange, but exactly resembling that made by the 2d solution; hence it seems probable, that an orange colour is only a deep yellow. Vitriolic æther readily dissolves the colouring part of this substance, and affords solutions of nearly the same colour as those made with spirit of wine. Oil of turpentine dissolves but a small portion of it, and acquires only a pale yellow colour. A solution of fixed alkali in water, digested with this substance, dissolves a large portion of its colouring part, and the solution is of a brownish yellow colour. Volatile spirit of sal ammoniac, seems to dissolve a larger portion of it than the fixed alkali, and the solution is of a reddish orange colour. A solution of soap in water, boiled with this substance, likewise dissolves its colouring part. All the foregoing solutions, except that in oil of turpentine, which was not tried, dye silk, cloth, and linen, of various shades of yellow and orange; but these colours are discharged, by boiling the dyed substances for some time in soap and water. This colour can therefore be of use only in dyeing silk and wool, for which purpose we are already furnished with good dyes. Few colours go so far in dyeing as this new substance, and none dye so speedily, especially when soap and water are used as the solvent; for a dip or two will dye cloth or silk of a lively yellow colour,

when put into the mixture while hot. Soap and water may be perhaps used with advantage, as the solvent for several other colours.

From the foregoing experiments it appears, that this colouring substance, on which they have been made, is of the resinous kind, and has a good deal of affinity with annotta.

*X. Experiments and Observations on the Gymnotus Electricus, or Electrical Eel. By Hugh Williamson, M. D., of Philadelphia. p. 94.*

A sea-faring man brought to this city a large eel, that had been caught in the province of Guiana, a little to the westward of Surinam. It had the extraordinary power of communicating a painful sensation, like that of an electrical shock, to people who touched it, and of killing its prey at a distance. The eel was 3 feet 7 inches long, and about 2 inches thick near the head. On a transient view it resembled one of our common eels both in shape and colour; but its head was flat, and its mouth wide, like that of a cat-fish, without teeth. A fin, which was above 2 inches broad, extended along its belly, from the point of its tail to within 6 inches of its head. This fin was almost an inch thick where it adhered to the body; the upper part of it was muscular, but of a very different texture from the muscular part of the body; the difference was obvious to the touch, but Dr. W. had no opportunity of making any observations by dissecting the subject. It was a native of fresh water, and breathed at the interval of 3 or 4 minutes, by lifting its head to the surface.

*Exper. 1.* On touching the eel with one hand, Dr. W. perceived such a sensation in the joints of his fingers as he received on touching a prime conductor or charged phial, when no circle was formed; or such as he had received, when a few sparks of the electric fluid have been conveyed through his fingers only. *2.* On touching the eel more roughly, he perceived a similar effect in his wrist and elbow. *3.* Touching the eel with an iron rod, 12 inches long, he perceived the like sensation in the joints of the thumb and fingers with which he held the metal. *4.* While another person provoked the eel by touching it, Dr. W. put his hand into the water at the distance of 3 feet, and felt such a sensation in the joints of his fingers as when he had touched the eel, but not so painful. *5.* Some small fishes were thrown into the water where he was swimming; he killed them immediately, and swallowed them. *6.* A cat-fish,\* that was at least 1½ inch thick, was thrown into the water where the eel was swimming; he killed it also, and attempted to swallow it, but could not. *7.* To discover whether the eel killed those fish by an emission of the same fluid with which he affected the hand when touched, Dr. W. put his hand into the water, at some distance from the eel; another cat-fish was thrown into the water; the eel swam up to it, but

\* The Bayre de Rio of Marcgrave.—Orig.

presently turned away, without offering any violence. After some time he returned; when, seeming to view it for a few seconds, he gave it a shock, by which it instantly turned up its belly, and continued motionless; at that very instant Dr. W. felt such a sensation in the joints of his fingers as in experiment 4. 8. A third cat-fish was thrown into the water, to which the eel gave such a shock, that it turned on its side, but continued to give signs of life. The eel seeming to observe this, as it was turning away, immediately returned, and struck it quite motionless. It could easily be perceived that the last shock was more severe than the former. The eel never attempted to swallow any of those fish after the first, though he killed many of them; and whenever he was going to kill one, he swam directly up to it, as if he was going to bite it; when he came up, he sometimes paused before he gave the shock, at other times he gave the shock immediately. On removing any of those cat-fish, though apparently dead, into water in another vessel, they presently recovered. Fish that are stunned by a small electrical shock were found to recover in the same manner. 9. Touching the eel, so as to provoke it, with one hand, and at the same time holding the other hand in the water, at a small distance, a shock passed through both arms, as in the case of the Leyden-experiment. 10. Dr. W. put the end of a wet stick into the water, and holding it with one hand, he touched the eel with the other; a shock passed through both arms as before. 11. Taking another gentleman in company by the hand, he touched the eel while Dr. W. held one of his hands in the water; the shock passed through them both. 12. Instead of putting his hand into the water, at a distance from the eel, as in the last experiment, he touched its tail, so as not to offend it, while an assistant touched its head more roughly; they both received a severe shock. 13. Eight or 10 persons, taking hands, stood in a circular form; the first in the series touched the eel, while the last put his hand into the water, at some distance from it; they all received a gentle shock. 14. The above experiment was repeated with no other variation than that the last person touched the eel's tail, while the first touched its head; they all received a severe shock. 15. Another gentleman and Dr. W. holding the extremities of a brass chain, the one put his hand into the water while the other touched the eel, so as to offend it; the shock passed through both. 16. Dr. W. wrapped a silk handkerchief round his hand, and touched the eel with it, but received no shock; though another gentleman felt the shock, who, at the same time, put his hand into the water, at some distance from the eel. 17. A great variety of other experiments were made by 2 persons, one touching the eel near its head, the other putting his hand into the water, or touching it near the tail, forming a communication at the same time between their hands, which were out of the water, by pieces of charcoal, rods of iron or brass, a piece of dry wood, glass, silk, &c. The uniform result

of all these experiments was, that whatever usually conveys the electrical fluid, would also convey the fluid discharged by the eel; and vice versâ, a brass chain, that had very many links in it, would not convey it, unless when the shock was severe, or the chain tense. 18. One of the company being insulated on glass bottles, received several shocks from the eel; but he exhibited no marks of a plus state of electricity, nor would cork balls, suspended by silken threads, give any marks of it, either when they were suspended over the eel's back, or touched by the insulated person at the instant he received the shock. 19. A person, holding a phial in one hand properly lined and coated for electrical experiments, put his hand to the tail of the fish, while an assistant, holding a short wire in one hand, that communicated with the inside of the phial, grasped the fish near its head, so as to receive a severe shock in his hand and arm, but it passed no farther. 20. Two pieces of brass wire, about the thickness of a crow's quill, were screwed in opposite directions, into a frame of wood, so as to come within less than the 100th part of an inch of contact; they were rounded at the point. Dr. W. held the remote end of one of those wires, while an assistant held the other; in the mean while, one of them putting his hand into the water near the eel, the other touched it so as to receive a shock. They repeated this experiment 15 or 20 times with different success: when the points of the wires were even screwed asunder, to the 50th part of an inch, the shock never passed in the circle; but when they were screwed up within the thickness of double-post paper, the shocks, such of them as were severe, would pass through them both; in which case, they doubtless leaped from the point of one wire to the other, though he was not so fortunate as to render the spark generally visible. But it should be observed, that the eel, on which he made these experiments, was not easily provoked, and appeared to be in bad health. He frequently passed his hand along its back and sides from head to tail, and lifted part of its body above the water, without tempting it to make any defence. Dr. Bancroft says, that such eels in Guiana have shocked his hand at the distance of some inches from the surface of the water. Perhaps fire emitted by eels lately taken, might be rendered visible.

From the above experiments it appears: 1. That the Guiana eel has the power of communicating a painful sensation to animals that touch or come near it. 2. That this effect depends entirely on the will of the eel; that it has the power of giving a small shock, a severe one, or none at all, just as circumstances may require. 3. That the shock given, or the painful sensation communicated, depends not on the muscular action of the eel, since it shocks bodies in certain situations at a great distance; and since particular substances only will convey the shock, while others, equally elastic or hard, refuse to convey it. 4. That the shock must therefore depend on some fluid, which the eel discharges from



its body. 5. That as the fluid discharged by the eel affects the same parts of the human body that are affected by the electric fluid; as it excites sensations perfectly similar; as it kills or stuns animals in the same manner; as it is conveyed by the same bodies that convey the electric fluid, and refuses to be conveyed by other bodies that refuse to convey the electric fluid; it must also be the true electrical fluid; and the shock given by this eel, must be the true electrical shock.

While Dr. W. made these experiments, the eel was kept in a large vessel, supported by pieces of dry timber, about 3 feet above the floor. Perhaps it may deserve notice, that a small hole being bored in the vessel in which the eel was swimming, one person provoked the eel so as to receive a shock; another person at the same time, not in contact with him, but holding his finger in the stream that spouted from the vessel, received a shock also in that finger. From this and sundry other experiments, Dr. W. believes that the gymnotus has powers greatly superior to, or rather different from those of the torpedo.

*XI. Of the Gymnotus Electricus, or Electrical Eel. In a Letter from Alexander Garden, M. D., F. R. S. Dated Charles-Town, South Carolina, Aug. 14, 1774. p. 102.*

There are 5 of these fishes now here, of different sizes, from 2 feet in length to three feet 8 inches. The following description was made out from the longest and largest. It might have been much more accurate, if there had been a possibility of handling the fish, and examining it leisurely; or if there had been a dead specimen, as many things relating to the internal and external structure could in that case have been more exactly ascertained. But this fish has the amazing power of giving so sudden and so violent a shock to any person that touches it, that there seems an absolute impossibility of ever examining accurately a living specimen; and the person who owns them rates them at too high a price, not less than 50 guineas for the smallest, for me to get a dead specimen, unless one should die by accident.

The largest of these fish was, as before said, 3 feet 8 inches in length, when extending itself most, and from 10 to 14 inches in circumference about the thickest part of his body. The head is large, broad, flat, smooth, and impressed here and there with holes, as if perforated with a blunt needle, especially towards the sides, where they are more regularly ranged in a line on each side. The rostrum is obtuse and rounded. The upper and lower jaws are of an equal length, and the gape is large. The nostrils are two on each side; the first large, tubular, and elevated above the surface; and the others small, and level with the skin, placed immediately behind the verge of the rostrum, at the distance of an inch asunder. The eyes are small, flattish, and of a bluish colour, placed

about  $\frac{3}{4}$  of an inch behind the nostrils, and more towards the sides of the head. The whole head seems to be well supported; but whether with bones or cartilages, is uncertain. The body is large, thick, and roundish, for a considerable distance from the head, and then gradually grows smaller, but at the same time deeper, or becomes of an acinaciform shape, to the point of the tail, which is rather blunt. There are many light-coloured spots on the back and sides of the body, placed at considerable distances in irregular lines, but more numerous and distinct towards the tail. When the fish was swimming, it measured 6 inches in depth, near the middle, from the upper part of the back to the lower edge of the fin, and it could not be more than 2 inches broad on the back at that place. The whole body, from about 4 inches below the head, seems to be clearly distinguished into 4 different longitudinal parts or divisions. The upper part or back is roundish, of a dark colour, and separated from the other parts on each side by the lateral lines; which, taking their rise at the base of the head, just above the pectoral fins, run down the sides, gradually converging, as the fish becomes smaller, to the tail, and make so visible a depression or furrow in their course, as to distinguish this from the 2d part or division, which may be properly called the body, or at least, appears to be the strong muscular part of the fish. This second division is of a lighter and more clear bluish colour than the upper or back part, and seems to swell out somewhat on each side, from the depression of the lateral lines; but, towards the lower or under part, is again contracted, or sharpened into the 3d part, or carina. This carina, or heel, is very distinguishable from the other two divisions, by its thinness, its apparent laxness, and by the reticulated skin of a more grey and light colour, with which it is covered. When the animal swims gently in pretty deep water, the rhomboidal reticulations of the skin of this carina are very discernible; but when the water is shallow, or the depth of the carina is contracted, these reticulations appear like many irregular longitudinal plicæ. The carina begins about 6 or 7 inches below the base of the head, and gradually widening or deepening as it goes along, reaches down to the tail, where it is thinnest. It seems to be of a strong muscular nature. Where it first takes its rise from the body of the fish, it seems to be about 1 inch or  $1\frac{1}{4}$  inches thick, and is gradually sharpened to a thin edge, where the 4th and last part is situated; videlicet, a long, deep, soft, wavy fin, which takes its rise about 3 or 4 inches at most below the head, and runs down along the sharp edge of the carina to the extremity of the tail. Where it first rises it is not deep, but gradually deepens or widens as it approaches to the tail. It is of a very pliable soft consistence, and seems rather longer than the body. The situation of the anus in this fish is very singular, being placed underneath, and being about an inch more forward than the pectoral fins, and consequently considerably nearer the rostrum. It is a pretty long rima

in appearance; but the aperture must be very small, as the formed excrements are only about the size of a quill of a common dunghill fowl. There are two pectoral fins, placed one on each side, just behind the head, over the foramina spiratoria, which are small, and generally covered with a lax skin, situated in the axillæ of these fins. These fins are small for the size of the fish, being scarcely an inch in length, of a very thin, delicate consistence, and orbicular shape. They seem to be chiefly useful in supporting and raising the head of the fish when he wants to breathe, which he does every 4 or 5 minutes, by raising his mouth out of the water. This shows that he has lungs and is amphibious, and the foramina spiratoria seem to indicate his having branchiæ likewise. Dr. G. mentions the appearances of a number of small cross bands, annular divisions, or rather rugæ of the skin of the body. They reach across the body down to the base of the carina on each side; but those that cross the back seem to terminate at the lateral lines, where new rings take their rise, not exactly in the same line, and run down to the carina. This gives the fish somewhat of a worm-like appearance; and indeed it seems to have some of the properties of this tribe, for it has a power of lengthening or shortening its body to a certain degree, for its own conveniency, or agreeable to its own inclination: and besides this power of lengthening or shortening his body, he can swim forwards or backwards with apparently equal ease to himself, which is another property of the vermicular tribe. When he swims forward, the undulation or wavy motion of the fin and carina begin from the upper part, and move downwards; but when he swims backward, and the tail goes foremost, the undulations of the fin begin at the extremity of the tail or fin, and proceed in succession from that backwards to the upper part of the body; in either case he swims equally swift. Every now and then the fish lays himself on one side, as it were, to rest himself, and then the 4 several divisions of his body abovementioned are very distinctly seen; videlicet, the vermiform appearance of the 2 upper divisions; the retiform appearance of the carina; and the last, or dark-coloured fin, whose rays seem to be exceedingly soft and flexible, and entirely at the command of the strong muscular carina. When he is taken out of the water, and laid on his belly, the carina and fin lie to one side, in the same manner as the ventral fin of the tetraodon does, when he creeps on the ground.

The person to whom these animals belong, calls them electrical fish; and indeed the power they have of giving an electrical shock to any person, or to any number of persons who join hands together, the extreme person on each side touching the fish, is their most singular and astonishing property. All the 5 are possessed of this power in a very great degree, and communicate the shock to one person, or to any number of persons, either by the immediate touch of the fish with the hand, or by the mediation of any metalline rod. The keeper says,

that when they were first caught, they could give a much stronger shock by a metalline conductor than they can do at present. The person who is to receive the shock must take the fish with both hands, at some considerable distance asunder, so as to form the communication, otherwise he will not receive it; at least he never saw any one shocked from taking hold of it with one hand only: though some have assured him that they were shocked by laying one hand on him. When it is taken hold of with one hand, and the other hand is put into the water over its body, without touching it, the person receives a smart shock; and the same effect follows, when a number join hands, and the person at one extremity of the circle takes hold of, or touches the fish, and the person at the other extremity puts his hand into the water, over the body of the fish. The shock was communicated through the whole circle, as smartly as if both the extreme persons had touched the fish. In this it seems to differ widely from the torpedo, or else we are much misinformed of the manner in which the benumbing effect of that fish is communicated. The shock which our Surinam fish gives, seems to be wholly electrical; and all the phenomena or properties of it exactly resemble those of the electric aura of our atmosphere when collected, as far as they are discoverable from the several trials made on this fish. This stroke is communicated by the same conductors, and intercepted by the interposition of the same original electrics, or electrics per se, as they used to be called. The keeper of this fish says, that he caught them in Surinam river, a great way up, beyond where the salt water reaches; and that they are a fresh water fish only. He says, that they are eaten, and by some people esteemed a great delicacy. They live on fish, worms, or any animal food, if it is cut small, so that they can swallow it. When small live fishes are thrown into the water, they first give them a shock, which kills or so stupifies them, that they can swallow them easily, and without any trouble. If one of these small fishes, after it is shocked, and to all appearance dead, be taken out of the vessel where the electrical fish is, and put into fresh water, it will soon revive again. If a larger fish than they can swallow be thrown into the water, at a time that they are hungry, they give him some smart shocks, till he is apparently dead, and then they try to swallow or suck him in; but, after several attempts, finding he is too large, they quit him. On the most careful inspection of such fish, Dr. G. could never see any mark of teeth, or the least wound or scratch on them. When the electrical fish are hungry, they are pretty keen after their food; but they are soon satisfied, not being able to contain much at one time. An electrical fish of 3 feet and upwards in length cannot swallow a small fish above 3, or at most  $3\frac{1}{4}$  inches long. I have had Mr. Bancroft's Essay on the Natural History of Guiana put into my hands, in which I find an account of this animal; but, as I think that he has not been very particular in the description of it, I

resolved still to send you the above account, that you might judge for yourself. I observe, that his account or description and mine differ in several things; and among others, where he says, that those fish were usually about 3 feet in length; but the one, of which I have sent a slight description, was 3 feet 8 inches. He was told, that some of these fish have been seen in Surinam river, upwards of 20 feet long, whose stroke or shock proved instant death to any person that unluckily received it.

*XII. Experiments and Observations in a Heated Room. By Charles Blagden, M.D., F. R. S. p. 111.*

About the middle of January, several gentlemen, with Dr. B., received an invitation from Dr. George Fordyce, to observe the effects of air heated to a much higher degree than it was formerly thought any living creature could bear. They all rejoiced at the opportunity of being convinced, by their own experience, of the wonderful power with which the animal body is endued, of resisting a heat vastly greater than its own temperature; and their curiosity was not a little excited to observe the circumstances attending this remarkable power. They knew indeed, that of late several convincing arguments had been adduced, and observations made, to show the error of the common opinions on this subject; and that Dr. Fordyce had himself proved the mistake of Dr. Boerhaave\* and most other authors, by supporting many times very high degrees of heat, in the course of a long train of important experiments; with which he hoped Dr. F. himself would favour the public. In the mean time, Dr. B. was happy in an opportunity of laying before the R. S. the following short account of some of these experiments, and of the views with which they were undertaken; for the particulars of which he was obliged to Dr. Fordyce himself.

Dr. Cullen long ago suggested many arguments to show, that life itself had a power of generating heat, independent of any common chemical or mechanical means; for, before his time, the received opinions were, that the heat of animals arose either from friction or fermentation.† Governor Ellis, in the year 1758, observed,‡ that a man can live in air of a greater heat than that of his

\* Elem. Chemiæ, tom. I, p. 277, 278.—Orig.

† To do further justice to the philosophy of this most ingenious and respectable professor, Dr. B. declares, that during his stay in Edinburgh, from the year 1765 to 1769, the idea of a power in animals of generating cold (that was the expression) when the heat of the atmosphere exceeded the proper temperature of their bodies, was pretty generally received among the students of physic, from Dr. Cullen's arguments; in consequence of which he applied a thermometer, in a hot summer day, to the belly of a frog, and found the quicksilver sink several degrees: a rude experiment indeed, but serving to confirm the general fact, that the living body possesses a power of resisting the communication of heat.—Orig.

‡ Phil. Trans., vol. I. p. 755.—Orig.

body, and that the body in this situation, continues its own cold. The Abbé Chappe d'Auteroche informs us, that the Russians use their baths heated to  $60^{\circ}$ \* of Reaumur's thermometer, about 160 of Fahrenheit's, without taking notice however of the heat of their bodies when bathing. With a view to add further evidence to these extraordinary facts, and to ascertain the real effects of such great degrees of heat on the human body, Dr. Fordyce tried the following experiments.

He procured a suite of rooms, of which the hottest was heated by flues in the floor, and by pouring on it boiling water; and the 2d was heated by the same flues, which passed through its floor to the 3d. The first room was nearly circular, about 10 or 12 feet in diameter and height, and covered with a dome, in the top of which was a small window. The 2d and 3d rooms were square, and both furnished with a sky-light. There was no chimney in these rooms, nor any vent for the air, excepting through crevices at the door. In the first room were placed 3 thermometers; one in the hottest part of it, another in the coolest part, and a 3d on the table, to be used occasionally in the course of the experiment: the frame of this last was made to turn back by a joint, so as to leave the ball and about 2 inches of the stem quite bare, that it might be more conveniently applied for ascertaining the heat of the body, and several other purposes.

*Exper. 1.* In the first room the highest thermometer stood at  $120^{\circ}$ , the lowest at  $110^{\circ}$ ; in the 2d room the heat was from  $90^{\circ}$  to  $85^{\circ}$ ; the 3d room felt moderately warm, while the external air was below the freezing point. About 3 hours after breakfast, Dr. Fordyce having taken off all his clothes, except his shirt, in the 3d room, and being furnished with wooden shoes, or rather sandals tied on with list, entered into the 2d room, and staid 5 minutes in a heat of  $90^{\circ}$ , when he began to sweat gently. He then entered the 1st room, and stood in the part heated to  $110^{\circ}$ ; in about  $\frac{1}{4}$  a minute his shirt became so wet that he was obliged to throw it aside, and then the water poured down in streams over his whole body. Having remained 10 minutes in this heat of  $110^{\circ}$ , he removed to the part of the room heated to  $120^{\circ}$ ; and after staying there 20 minutes, he found that the thermometer placed under his tongue, and held in his hand, stood just at  $100^{\circ}$ , and that his urine was of the same temperature. His pulse had gradually risen till it made 145 pulsations in a minute. The external circulation was greatly increased; the veins had become very large, and a universal redness had diffused itself over the body, attended with a strong feeling of heat. His respiration however was but little affected. Here Dr. Fordyce remarks, that the moisture of his skin most probably proceeded chiefly from the condensation of the vapour in the room on his body. He concluded this experiment in the

\* Voy. en Sibirie, tom. i. p. 51.—Orig.

2d room, by plunging into water heated to  $100^{\circ}$ ; and after having been wiped dry, was carried home in a chair; but the circulation did not subside for 2 hours, after which he walked out in the open air, and scarcely felt the cold.

*Exper. 2.* In the first room the highest thermometer varied from  $132^{\circ}$  to  $130^{\circ}$ ; the lowest stood at  $119^{\circ}$ . Dr. Fordyce having undressed in an adjoining cold chamber, went into the heat of  $119^{\circ}$ ; in  $\frac{1}{4}$  a minute the water poured down in streams over his whole body, so as to keep that part of the floor where he stood constantly wet. Having remained here 15 minutes, he went into the heat of  $130^{\circ}$ ; at this time the heat of his body was  $100^{\circ}$ , and his pulse beat 126 times in a minute. While Dr. Fordyce stood in this situation, he had a Florence flask brought in, filled with water heated to  $100^{\circ}$ , and a dry cloth, with which he wiped the surface of the flask quite dry; but it immediately became wet again, and streams of water poured down its sides; which continued till the heat of the water within had risen to  $122^{\circ}$ , when Dr. Fordyce went out of the room, after having remained 15 minutes in a heat of  $130^{\circ}$ ; just before he left the room his pulse made 139 beats in a minute, but the heat under his tongue, in his hand, and of his urine, did not exceed  $100^{\circ}$ . Here Dr. Fordyce observes, that as there was no evaporation, but constantly a condensation of vapour on his body, no cold was generated but by the animal powers. At the conclusion of this experiment, Dr. Fordyce went into a room where the thermometer stood at  $43^{\circ}$ , dressed himself there, and immediately went out into the cold air, without feeling the least inconvenience; on which he remarks, that the transition from very great heat to cold is not so hurtful as might be expected, because the external circulation is so excited, as not to be readily overcome by the cold. Dr. Fordyce has since had occasion, in making other experiments, to go frequently into a much greater heat, where the air was dry, and to stay there a much longer time, without being affected nearly so much; for which he assigns 2 reasons; that dry air does not communicate its heat like air saturated with moisture; and that the evaporation from the body, which takes place when the air is dry, assists its living powers in producing cold. It must be immediately perceived that, besides the principal object, these curious experiments throw great light on many other important subjects of natural philosophy.

January 23. The Hon. Captain Phipps, Mr. Banks, Dr. Solander, and Dr. Blagden, attended Dr. Fordyce to the heated chamber, which had served for many of his experiments with dry air. They went in without taking off any of their clothes. It was an oblong-square room, 14 feet by 12 in length and width, and 11 in height, heated by a round stove, or cockle, of cast iron, which stood in the middle, with a tube for the smoke carried from it through one of the side walls. When they first entered the room, about 2 o'clock in the afternoon, the quicksilver in a thermometer, which had been suspended there, stood

above the 150th degree. By placing several thermometers in different parts of the room they afterwards found, that the heat was a little greater in some places than in others; but that the whole difference never exceeded  $20^{\circ}$ . They continued in the room above 20 minutes, in which time the heat had risen about  $12^{\circ}$ , chiefly during the first part of their stay. Within an hour afterwards, they went into this room again, without feeling any material difference, though the heat was considerably increased. On entering the room a 3d time, between 5 and 6 o'clock after dinner, they observed the quicksilver in their only remaining thermometer at  $198^{\circ}$ \*: this great heat had so warped the ivory frames of the other thermometers, that every one of them was broken. They now staid in the room, all together, about 10 minutes; but finding that the thermometer sunk very fast, it was agreed, that for the future only one person should go in at a time, and orders were given to raise the fire as much as possible. Soon afterwards Dr. Solander entered the room alone, and saw the thermometer at  $210^{\circ}$ ; but, during 3 minutes that he staid there, it sunk to  $196^{\circ}$ . Another time, he found it almost 5 minutes before the heat was lessened from  $210^{\circ}$  to  $196^{\circ}$ . Mr. Banks closed the whole, by going in when the thermometer stood above  $211^{\circ}$ ; he remained 7 minutes, in which time the quicksilver had sunk to  $198^{\circ}$ ; but cold air had been let into the room, by a person who went in and came out again during Mr. Banks's stay. The air heated to these high degrees felt unpleasantly hot, but was very bearable. Their most uneasy feeling was a sense of scorching on the face and legs; their legs particularly suffered very much, by being exposed more fully than any other part to the body of the stove, heated red hot by the fire within. Their respiration was not at all affected; it became neither quick nor laborious; the only difference was a want of that refreshing sensation which accompanies a full inspiration of cool air. Their time was so taken up with other observations that they did not count their pulses by the watch: Dr. Blagden's, to the best of his judgment by feeling it, beat at the rate of 100 pulsations in a minute, near the end of the first experiment; and Dr. Solander's made 92 pulsations in a minute soon after they had gone out of the heated room. Mr. Banks sweated profusely, but no one else; Dr. Blagden's shirt was only damp at the end of the experiment. But the most striking effects proceeded from their power of preserving their natural temperature. Being now in a situation in which their bodies bore a very different relation to the surrounding atmosphere from that to which they had been accustomed, every moment presented new phenomena. Whenever they breathed on a thermometer, the quicksilver sunk several degrees. Every expiration, particularly if made with any degree of violence, gave a very pleasant

\* This thermometer stands, near the boiling point, about 1 degree too high.—Orig.



impression of coolness to their nostrils, scorched just before by the hot air rushing against them when they inspired. In the same manner their now cold breath agreeably cooled their fingers whenever it reached them. On touching his side, Dr. B. says, it felt cold like a corpse; and yet the actual heat of his body, tried under his tongue, and by applying closely the thermometer to his skin, was  $98^{\circ}$ , about a degree higher than its ordinary temperature. When the heat of the air began to approach the highest degree which this apparatus was capable of producing, their bodies in the room prevented it from rising any higher; and when it had been previously raised above that point, inevitably sunk it. Every experiment furnished proofs of this: toward the end of the first, the thermometer was stationary: in the 2d, it sunk a little during the short time they staid in the room: in the 3d, it sunk so fast as to oblige them to determine that only one person should go in at a time: and Mr. Banks and Dr. Solander each found, that his single body was sufficient to sink the quicksilver very fast, when the room was brought nearly to its maximum of heat.

These experiments therefore prove, in the clearest manner, that the body has a power of destroying heat. To speak justly on this subject, it must be called a power of destroying a certain degree of heat communicated with a certain quickness. Therefore in estimating the heat which we are capable of resisting, it is necessary to take into consideration not only what degree of heat would be communicated to our bodies, if they possessed no resisting power, by the heated body, before the equilibrium of heat was effected; but also what time the heat would take in passing from the heated body into our bodies. In consequence of this compound limitation of our resisting power, we bear very different degrees of heat in different mediums. The same person who felt no inconvenience from air heated to  $211^{\circ}$ , could not bear quicksilver at  $120^{\circ}$ , and could just bear rectified spirit of wine at  $130^{\circ}$ ; that is, quicksilver heated to  $120^{\circ}$  furnished, in a given time; more heat for the living powers to destroy, than spirits heated to  $130^{\circ}$ , or air to  $211^{\circ}$ .\* And they had, in the heated room where their experiments were made, a striking though familiar instance of the same. All the pieces of metal there, even their watch chains, felt so hot, that they could scarcely bear to touch them for a moment, while the air, from which the metal had derived all its heat, was only unpleasant. The slowness with which air communicates its heat was further shown, in a remarkable manner, by

\* These numbers are the result of some experiments which were made on the first of February, in a room where the heat of the air was  $65^{\circ}$ . Mr. Banks and Dr. Blagden found that they could bear spirits which had been considerably heated and were then cooling, when the thermometer came to the 130th degree; cooling oil at  $129^{\circ}$ ; cooling water at  $123^{\circ}$ ; cooling quicksilver at  $117^{\circ}$ . And these points were pretty nicely determined; so that though they could bear water very well at  $123^{\circ}$ , they could not bear it at  $125^{\circ}$ , an experiment in which Dr. Solander joined them. And their feelings with respect to all these points, seemed pretty exactly the same.—Orig.

the thermometers they brought with them into the room, none of which at the end of 20 minutes, in the first experiment, had acquired the real heat of the air by several degrees. It might be supposed, that by an action so very different from that to which the human body is accustomed, as destroying a large quantity of heat, instead of generating it, they must have been greatly disordered. And indeed they experienced some inconvenience; their hands shook very much, and they felt a considerable degree of languor and debility; Dr. B. had also a noise and giddiness in his head. But it was only a small part of their bodies that exerted the power of destroying heat with such a violent effort as seems necessary at first sight. Their clothes, contrived to guard them from cold, guarded them from the heat on the same principles. Underneath they were surrounded with an atmosphere of air, cooled on one side to  $98^{\circ}$ , by being in contact with their bodies, and on the other side heated very slowly; because woollen is such a bad conductor of heat. Accordingly Dr. B. found, toward the end of the first experiment, that a thermometer put under his clothes, but not in contact with his skin, sunk down to  $110^{\circ}$ . On this principle it was that the animals, subjected by M. Tillet to the interesting experiments related in the *Memoirs of the Academy of Sciences* for the year 1764, bore the oven so much better when they were clothed, than when they were put in bare: the heat actually applied to the greatest part of their bodies was considerably less in the first case than in the last. As animals can destroy only a certain quantity of heat in a given time, so the time they can continue the full exertion of this destroying power seems to be also limited; which may be one reason why we can bear for a certain time, and much longer than can be necessary to fully heat the cuticle, a degree of heat which will at length prove intolerable. Probably both the power of destroying heat, and the time for which it can be exerted, may be increased, like most other faculties of the body, by frequent exercise. It might be partly on this principle that, in M. Tillet's experiments, the girls who had been used to attend the oven bore, for 10 minutes, a heat which would raise Fahrenheit's thermometer to  $270^{\circ}$ : in the above experiments however, not one of them thought he suffered the greatest degree of heat that he was able to support.

A principal use of all these facts is, to explode the common theories of the generation of heat in animals. No attrition, no fermentation, or whatever else the mechanical and chemical physicians have devised, can explain a power capable of producing or destroying heat, just as the circumstances of the situation require. A power of such a nature, that it can only be referred to the principle of life itself, and probably exercised only in those parts of our bodies in which life seems peculiarly to reside. From these, with which no considerable portion of the animal body is left unprovided, the generated heat may be readily communicated to every particle of inanimate matter that enters into our composition.

This power of generating heat seems to attend life very universally. Not to mention other well known experiments, Mr. Hunter found a carp preserve a coat of fluid water round him, long after all the rest of the water in the vessel had been congealed by a very strong freezing mixture. And as for insects, Dr. Martine\* observed, that his thermometer, buried in the midst of a swarm of bees, rose to  $97^{\circ}$ . It seems extremely probable, that vegetables, together with the many other vital powers which they possess in common with animals, have something of this property of generating heat. Dr. B. doubts if the sudden melting of snow which falls upon grass, while that on the adjoining gravel walk continues so many hours unthawed, can be adequately explained on any other supposition. Moist dead sticks are often found frozen quite hard, when in the same garden the tender growing twigs are not at all affected. And many herbaceous vegetables, of no great size, resist every winter degrees of cold which are found sufficient to freeze large bodies of water. It may be proper to add, that after each of the abovementioned experiments of bearing high degrees of heat, they went out immediately into the open air, without any precaution, and experienced from it no bad effect. The languor and shaking of their hands soon went off, and they did not afterwards suffer the least inconvenience.

*XIII. The supposed Effect of Boiling on Water, in disposing it to Freeze more readily, ascertained by Experiments. By Joseph Black, M. D.,† Professor of Chemistry at Edinburgh. p. 124.*

“We had lately, says Dr. Black, one day of a calm and clear frost; and I immediately seized the opportunity, which I missed before, to make some

\* *Essays Medical and Philosophical*, p. 331.—Orig.

† This celebrated chemical philosopher was born in 1728, at Bourdeaux, in France, of British parents. He was sent for education first to Belfast, and afterwards to Glasgow, where he studied physic and took the degree of M. D. He was afterwards appointed to read lectures on chemistry and medicine in that university; and in 1766 Dr. Cullen having exchanged the professorship of chemistry for that of the practice of physic in the university of Edinburgh; Dr. Black was appointed to succeed him; and the duties of this office he continued to discharge with increasing reputation for upwards of 30 years. In this situation, says Professor Robison, he soon became one of the principal ornaments of the university of Edinburgh, and his lectures were attended by a crowded audience. It could not be otherwise. His personal appearance and manners were those of a gentleman, and peculiarly pleasing. His voice in lecturing was low but fine; and his articulation so distinct, that he was perfectly well heard by an audience consisting of several hundreds. His discourse was so plain and perspicuous, his illustration by experiment so apposite, that his sentiments on any subject never could be mistaken; and his instructions were so clear of all hypothesis or conjecture, that the hearer rested on his conclusions with a confidence scarcely exceeded in matters of his own experience.

Dr. B.'s constitution was never strong, and for many years preceding his death, he had been subject to a spitting of blood, which he had prevented from proceeding to an alarming length by a very abstemious diet and remarkable serenity of mind. His bodily strength, however, declined very visibly

experiments relative to the freezing of boiled water, in comparison with that of water not boiled. I ordered some water to be boiled in the tea kettle 4 hours. I then filled with it a Florentine flask, and immediately applied snow to the flask, till I cooled it to  $48^{\circ}$  of Fahrenheit, the temperature of some unboiled water which stood in my study in a bottle; then putting 4 oz. of boiled, and 4 of the unboiled water, separately, into 2 equal tea cups, I exposed them on the outside of a north window, where a thermometer pointed to  $29^{\circ}$ . The consequence was, that ice appeared first on the boiled water; and this, in several repetitions of the experiment, with the same boiled water, some of which were made 9 hours after it was poured out of the tea kettle. The length of time which intervened between the first appearance of ice on the 2 waters, was different in the different experiments. One cause of this variety was plainly a variation of the temperature of the air, which became colder in the afternoon, and made the thermometer descend gradually to  $25^{\circ}$ . Another cause was the disturbance of the water; when the unboiled water was disturbed now and then by stirring it gently with a quill tooth-pick, the ice was formed on it as soon, or very nearly as soon, as on the other; and from what I saw, I have reason to think, that were it to be stirred incessantly, provided at the same time the experiment were made with quantities of water, not much larger or deeper than these, it would begin to freeze full as soon. In one of these trials, having inspected my tea cups

during 1798 and 1799; and on the 26th of Nov. of the last-mentioned year he expired suddenly, while at table, with his usual fare, some bread, a few prunes, and a measured quantity of milk diluted with water. He had the cup in his hand when the last stroke of his pulse was to be given, and had set it down on his knees, which were joined together, and had kept it steady with his hand, in the manner of a person perfectly at his ease: and in this attitude he expired, without spilling a drop, and without a writhe on his countenance. His servant thought he had been asleep. This euthanasia happened when he was in his 71st year.

When we take a view of Dr. B.'s experiments on magnesia and quicklime proving that the causticity in burnt lime and alkalies, is owing to their being deprived of fixed air (carbonic acid) with which they are combined in their mild state; and of the experiments which he made on the conversion of water into steam, showing the difference between sensible and latent heat, (in which originated the great improvements made by his pupil Mr. Watt in that admirable and most useful mechanical apparatus, the steam engine) when we take a view of these experiments, from whence as, from a centre, have radiated the brilliant discoveries in pneumatic chemistry of several contemporary philosophers, we shall be fully satisfied that they who have pronounced Dr. B. to have been one of the greatest chemists of the 18th century, have by no means over-rated his scientific character.

Besides his inaugural dissertation *De Acido a Cibis Orto*, and his Experiments on Quicklime above-mentioned, and the present paper in the Phil. Trans., Dr. B. published an Analysis of the Waters of some Boiling Springs in Iceland, (see the Trans. of the R. S. of Edinburgh). And after his death the world was favoured with the publication of his Lectures on Chemistry, in 2 vols. 4to., 1803, by his intimate friend Mr. Robison, Professor of Natural and Experimental Philosophy in Edinburgh; from whose and Dr. Ferguson's account of the author, the above particulars have been taken.

when they had been an hour exposed, and finding ice on the boiled water, and none on the other, I gently stirred the unboiled water with my tooth-pick, and saw immediately fine feathers of ice formed on its surface, which quickly increased in size and number, till there was as much ice in this cup as in the other, and all of it formed in one minute of time, or 2 at most. And in the rest of the trials, though the congelation began in general later in the unboiled water than in the other; when it did begin in the former, the ice quickly increased so as, in a very short time, to equal, or nearly equal in quantity, that which had been formed more gradually in the boiled water. The opinion, therefore, which I have formed from what I have hitherto seen is, that the boiled and common water differ from one another in this respect; that whereas the common water, when exposed in a state of tranquillity to air that is a few degrees colder than the freezing point, may easily be cooled to the degree of such air, and still continue perfectly fluid, provided it still remain undisturbed: the boiled water, on the contrary, cannot be preserved fluid in these circumstances; but when cooled down to the freezing point, if we attempt to make it in the least colder, a part of it is immediately changed into ice; after which, by the continued action of the cold air on it, more ice is formed in it every moment, till the whole of it be gradually congealed before it can become as cold as the air that surrounds it. From this discovery it is easy to understand, why they find it necessary to boil the water in India, in order to obtain ice. The utmost intensity of the cold which they can obtain by all the means they employ, is probably not greater than  $31^{\circ}$  or  $30^{\circ}$  of Fahrenheit's thermometer. Common water, left undisturbed, will easily descend to this degree without freezing; and, if they have not the means of making it colder, may continue fluid for any time, provided it be not disturbed: the refrigerating causes of that part of the world when they have done so much, have done their utmost, and can act no further on the water. But this cannot happen to the boiled water; when the refrigerating causes have cooled it to  $32^{\circ}$ , the next effect they produce, is to occasion in it the beginning of congelation, while the water is afterwards gradually assuming the form of ice, we know, by experience, that its temperature must remain at  $32^{\circ}$ ; it cannot be made colder, so long as any considerable part of it remains unfrozen.\* The refrigerating causes continue therefore to have power over it, and to act upon it, and will gradually change the whole into ice, if their action be continued sufficiently long.

The next object of investigation may be the cause of this difference between the boiled and the common water. In considering this point, the following idea

\* Common water, when cooled in a state of tranquillity to several degrees below the freezing point, will suddenly rise up to it again, if disturbed in such a manner as to occasion in it a beginning of congelation.—Orig.

was suggested. As we know from experience, that by disturbing common water, we hasten the beginning of its congelation, or render it incapable of being cooled below  $32^{\circ}$ , without being congealed; may not the only difference between it and boiling water, when they are exposed together to a calm frosty air, consist in this circumstance; that the boiled water is necessarily subjected to the action of a disturbing cause, during the whole time of its exposure, which the other is not? One effect of boiling water long, is to expel the air which it naturally contains; as soon as it cools, it begins to attract and absorb air again, till it has recovered its former quantity; but this probably requires a considerable time. During the whole of this time, the air entering into it must occasion an agitation or disturbance in the water, which, though not sensible to the eye, may be very effectual in preventing it to become, in the least, colder than the freezing point, without beginning to freeze, in consequence of which, its congelation must begin immediately after it is cooled to that point. When I reflect on this idea, I remember a fact which appears to me to support it strongly. Fahrenheit was the first person who discovered that water, when preserved in tranquillity, may be cooled some degrees below the freezing point without freezing. He made the discovery while he was endeavouring to obtain ice from water that had been purged of its air: with this intention he had put some water into little glass globes, and having purged it of air, by boiling and the air-pump, he suddenly sealed up the globes, and then exposed them to the frosty air. He was surprized to find the water remain unfrozen much longer than he expected, when at last he opened some of his globes, in order to apply a thermometer to the water, or otherwise examine what state it was in. The immediate consequence of the admission of the air was a sudden congelation, which happened in the water; and in the rest of his globes, a similar production of ice was occasioned by shaking them. The inference that may be drawn from these experiments of Fahrenheit's, is sufficiently obvious; it appears to remove all doubt with regard to the above supposition. Before these experiments of Fahrenheit occurred to my memory, I had planned a few, suggested by the above supposition, that might have led to the same conclusion; but the short duration of the frost, for one day only, did not give me time to put them in execution.

*XIV. Experiments on the Dipping-Needle, made by Desire of the R. S. By Thomas Hutchins. p. 129.*

In these experiments the instrument was placed in 4 several positions, viz. with the index placed east, and then placed west, with the poles of the needle placed one way, and then the same with the poles changed or reversed. In each

of these 4 positions, the dip was taken and noted down 3, 4, or 5 times. And the mediums of all these, for the several places, are as follow.

1. At Stromness in the isles of Orkney, lat.  $58^{\circ} 59'$  N., long.  $3^{\circ} 30'$  W. from London, June 9, 1774. The mean dip was  $75^{\circ} 51'$ .

In these observations the needle was placed horizontal, and the vibration continued between 9 and 10 minutes. The instrument was set in the middle of a room up one pair of stairs; but being apprehensive that the iron grate, fender, poker, and tongs, might, in some measure, affect the needle, trial was made in the open air, and in a place free from such obstacles.

2. On the Holms in the entrance of Stromness Harbour, June 23, 1774. Variation per azimuth  $24^{\circ}$  westerly. Long. from London  $3^{\circ} 30'$  W. lat.  $58^{\circ} 59'$  N. Dip.  $75^{\circ} 40'$ .

The needle in all these observations was left to vibrate from an horizontal position. The instrument was set on the top of the case in which it was packed, and stood in the open air, in a fine sunny day.

3. In Hudson's Straits, July 23, 1774, lat  $62^{\circ} 3'$  N., long.  $69^{\circ}$  W. from London, variation  $43^{\circ}$  westerly. Mean dip,  $82^{\circ} 42'$ .

The needle vibrated from an horizontal situation. These observations were made on a large piece of ice, to which the 3 ships were grappled.

4. In Hudson's Straits, July 27, 1774, lat  $62^{\circ} 23'$  N., long.  $71^{\circ} 30'$  W. from London, variation  $42^{\circ} 50'$  westerly per azimuth. Dip  $83^{\circ} 11'$ .

5. In Hudson's Straits, July 28, 1774, lat.  $62^{\circ} 25'$  N., long.  $71^{\circ} 30'$  W. from London, variation per azimuth  $44^{\circ}$  W. Dip  $82^{\circ} 46'$ .

6. In Hudson's Bay, August 14, 1774, lat.  $50^{\circ} 53'$  N., long.  $85^{\circ} 22'$  W. from London, variation per azimuth  $24^{\circ}$  W. Dip.  $82^{\circ} 41'$ .

These experiments were made in the cabin of the Prince Rupert, while she lay among ice. The ship frequently varied the position of her head a point of the compass; but by replacing the instrument as often as was found necessary, there was the greatest reason to think these observations, which took up above 3 hours, are pretty accurate.

7. At Moose Fort in Hudson's Bay, September 8, 1774, lat.  $51^{\circ} 20'$  N., long.  $82^{\circ} 30'$  W. from London, variation  $17^{\circ}$  W. Dip  $80^{\circ} 13'$ .

The observations were made on shore. So remarkable a difference between them, when Mr. H. was expecting quite the reverse, surprized him as much as the increased inclination of the needle from observations made nearly in the same parallel of latitude in London. He endeavoured, by drawing a magnetical meridional line with chalk, and paying the greatest attention to keeping the instrument perfectly steady and horizontal, to render these experiments accurate, and fulfil the intention of the R. S.

8. At Albany Fort, in Hudson's Bay, Sept. 14, 1774, long.  $82^{\circ} 30'$  w., lat.  $52^{\circ} 22'$  N., variation  $17^{\circ}$  w. Dip  $60^{\circ} 2'$ .

*Observations on Hoy 1774.*

Month.	Hour.	Barometer.	Thermometer.	Weather.	Circumstances.
June 11, 1774.	0 15	28.63	59	Clear.	On the top of the hill.
	0 30	28.60	56½	Foggy.	Ditto.
	4 15	30.22	63	Clear.	At low water mark.

Hoy is a remarkable high hill near Stromness, in the Orkneys, and is placed by Mr. Mackenzie in lat.  $58^{\circ} 58'$  N., and long.  $3^{\circ} 30'$  w. from London. The first 2 observations were made on the highest part of the hill. Soon after the first, a fog was seen below arising from the water, at length it reached the summit of the hill; the air seemed very raw and cold to the touch, and the instruments showed as in the 2d observation. The barometer continued at 28.60 inches after the fog was gone off, but the thermometer rose 2 or 3 degrees. The last observation was made at low water mark, about half a mile from the bottom of the hill. THOMAS HUTCHINS.

"The height of Hoy above low water mark according to these observations should be 249.93 fathoms, or as near as may be 500 yards, neglecting the correction for the difference that may be supposed in the temperature of the quicksilver at the two stations, the quantity of which is uncertain." S. HORSLEY.

*XV. A Meteorological Journal for the Year 1774, kept at the Royal Society's House by Order of the President and Council. p. 139.*

The observations of the barometer and thermometers were made two times on every day of the year, viz. at 8 o'clock in the morning, and about 2 afternoon. And the numbers collected for the several months were as in the following table.

1774	Thermometer without.					Thermometer within.			Barometer.			Rain.
	Greatest height.	Least height.	Mean height. A. M.	Mean height. P. M.	Mean whole day.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Inches.
January	50.5	24.0	30.0	37.7	35.3	50.5	27.0	37.4	30.17	28.79	29.57	2.958
February	52.0	24.5	37.6	43.5	40.5	50.0	33.0	42.4	30.46	29.16	29.81	2.360
March	60.0	33.5	39.6	50.5	45.0	61.5	38.0	47.4	30.33	29.14	29.82	1.780
April	67.0	36.5	44.9	54.8	49.8	60.0	45.0	51.3	30.24	29.33	29.79	1.242
May	69.0	45.0	49.7	59.9	54.8	51.5	51.0	55.9	30.18	29.34	29.87	1.413
June	77.5	52.0	59.1	68.4	63.7	71.0	59.0	64.6	30.34	29.47	29.90	2.237
July	83.5	55.5	59.7	70.1	64.9	78.5	60.0	65.9	30.36	29.61	30.00	2.438
August	78.0	73.0	58.2	69.2	63.7	78.0	52.5	54.6	30.32	29.38	29.95	3.340
September	73.0	42.5	52.6	62.1	57.3	69.5	49.5	59.7	30.28	29.11	29.79	3.743
October	64.5	36.0	46.0	56.3	51.1	61.0	45.5	53.6	30.57	29.40	30.13	1.348
November	58.5	41.0	39.2	43.5	41.3	56.0	34.5	43.7	30.22	29.17	29.81	1.627
December	53.5	25.0	37.3	40.8	39.7	49.0	30.5	40.8	30.71	29.11	30.09	1.826

Means of all ..... 50.6 ..... 52.4 ..... 29.88.. 26.312

The quantity of rain in the whole year was 26.312, or about  $26\frac{1}{2}$  inches.

*For the Variation of the Magnetic Needle.*—These observations were 4 times every day, from August 21 to Sept. 5, viz. in the morning, at noon, at 2 afternoon, and in the evening; the means of all which are respectively as below.

Morn.  $21^{\circ} 25'$ .... noon  $21^{\circ} 33'$ .... 2 p.m.  $21^{\circ} 28'$ .... even.  $21^{\circ} 17'$



The mean of all.....  $21^{\circ} 26'$   
 Error of instrument .....  $-10$   
 Correct variation. . . . .  $21 \quad 16$  west.

*XVI. An Abridged State of the Weather at London in the Year 1774, collected from the Meteorological Journal of the R. S. By S. Horsley, LL.D., Sec. R. S. p. 167.*

Though the practice of keeping meteorological journals is, of late years, become very general, no information of any importance has yet been derived from it. The reason of which perhaps may be, that after great pains and attention bestowed in registering particulars, as they occur, with a scrupulous minuteness, observers have not taken the trouble to form, at proper intervals of time, compendious abstracts of their records, exhibiting the general result of their observations in each distinct branch of meteorology. The following tables are given as an example of the method that may be taken in future to remedy this neglect. With the general state of the barometer and thermometer, already given at the end of the meteorological journal, they form a history of the weather at London during the last year. If the example were to be followed, in different parts of the kingdom, we might in time be furnished with an experimental history of the weather of our island.

TABLE I.

*An abridged View of the Winds at London, in 1774.*  
 Compiled from the Meteorological Journal of the Royal Society.

	N.	S.	E.	W.	N.W.	S.E.	N.E.	S.W.	Days	Rain.	
January	1½	0	2	1½	4	2½	4	13	31	2.958	{ Five half days omitted in the Journal.
February	1	1½	1	3	2	0	3½	16	28	2.360	
March	2	1½	1	1	½	3	14	7½	31	1.780	{ Half a day missed in the Journal.
April	2½	3	2	4½	2½	2	5	8½	30	1.242	
May	3	½	3½	0	6	3	10½	4	31	1.413	{ A half day missed in the Journal.
June	½	3½	2½	2½	4	2	1½	13½	30	2.273	
July	½	2	0	1	6½	2	1	18	31	2.438	
August	2	1½	0	1½	2	4	6	14	31	3.340	
September	1½	2½	1	2½	4	3½	6	9	30	3.743	
October	1	2½	1	2	3½	3	8	10	31	1.348	
November	7	1	1½	0	2½	1½	7	9½	30	1.627	
December	2½	1½	2	4½	6	3½	7½	3½	31	1.826	
	25	21	17½	24	43½	30	74	126½		26,312	

This table shows the number of days that each wind blew in each month, dividing the compass only into 8 points, and reckoning all the winds between N. and W., N. W.; all between S. and E., S. E.; all between N. and E., N. E.; and all

between s. and w. s. w. The number of days that each blew in all the months being collected into one sum at bottom, shows the number of days each wind blew in the whole year. The quantity of rain that fell in each month is added, that the connection between wet and dry, and the several winds may the more readily appear. It appears that the winds from the s. w. prevailed more than any other in the year 1774; and next to the s. w. the n. e. But the s. w. was more frequent than the n. e. in the proportion of 126 to 74. Of the winds from the 4 cardinal points, the n. was the most frequent, and the e. the most rare. In the 3 summer months, June, July, and August, there fell more rain than in the 3 of any other season. Of the 26.312 inches of rain which fell in the whole year, 13.826 fell in the winter half year, consisting of the 6 months of September, October, November, December, January, and February, and 12.486 in the summer half year, consisting of the 6 months of March, April, May, June, July, and August. So that the winter's rain exceeded the summer's by 1.340 inches; that is, by little more than  $\frac{1}{10}$  part of half the rain of the whole year. September gave the greatest quantity of rain, and April the least of any single month in the whole year.

TABLE II.

Sub-division of the s. w.

	w. s. w.	s. w.	s. s. w.	Sums.
January	2	9 $\frac{1}{2}$	1 $\frac{1}{2}$	18
February	4	7 $\frac{1}{2}$	4 $\frac{1}{2}$	16
March	1 $\frac{1}{2}$	4 $\frac{1}{2}$	1 $\frac{1}{2}$	7 $\frac{1}{2}$
April	3	3	2 $\frac{1}{2}$	8 $\frac{1}{2}$
May	$\frac{1}{2}$	2	1 $\frac{1}{2}$	4
June	1	9	3 $\frac{1}{2}$	13 $\frac{1}{2}$
July	5	9	4	18
August	5 $\frac{1}{2}$	4 $\frac{1}{2}$	4	14
September	2	1 $\frac{1}{2}$	5 $\frac{1}{2}$	9
October	2	5	3	10
November	2 $\frac{1}{2}$	5	2	9 $\frac{1}{2}$
December	1	2	$\frac{1}{2}$	3 $\frac{1}{2}$
Sums	30	62 $\frac{1}{2}$	34	126 $\frac{1}{2}$

TABLE III.

Sub-division of the n. e.

	e. n. e.	n. e.	n. n. e.	Sums.
January	2 $\frac{1}{2}$	1 $\frac{1}{2}$	1	4
February	0	2 $\frac{1}{2}$	1	3 $\frac{1}{2}$
March	2 $\frac{1}{2}$	9	2 $\frac{1}{2}$	14
April	$\frac{1}{2}$	3 $\frac{1}{2}$	1	5
May	2	8 $\frac{1}{2}$	0	10 $\frac{1}{2}$
June	0	$\frac{1}{2}$	1	1 $\frac{1}{2}$
July	0	$\frac{1}{2}$	$\frac{1}{2}$	1
August	2	1	3	6
September	1	1	4	6
October	2 $\frac{1}{2}$	3 $\frac{1}{2}$	2	8
November	4	1 $\frac{1}{2}$	1 $\frac{1}{2}$	7
December	1 $\frac{1}{2}$	3 $\frac{1}{2}$	2 $\frac{1}{2}$	7 $\frac{1}{2}$
Sums	18 $\frac{1}{2}$	35 $\frac{1}{2}$	20	74

In these 2 tables the winds between the w. and the s. w. are all set down to the w. s. w.; and those between the s. and the s. w. are all reckoned s. s. w. In like manner, the winds between the e. and n. e. are all reckoned e. n. e.; and those between the n. and n. e. are all reckoned n. n. e. It appears that of the winds between the s. and w. those from the point of s. w. were far more frequent than those from either side of it. And the winds from the point of n. e. more frequent than those on either side of it, nearly in the same proportion.

TABLE IV.

Sub-division of the s. e.

	E. S. E.	S. E.	S. S. E.	Sums.
January	1½	1	0	2½
February	0	0	0	0
March	½	1½	1	3
April	0	1	1	2
May	0	0	3	3
June	0	1½	½	2
July	½	0	1½	2
August	1	2½	½	4
September	0	1½	2	3½
October	½	1½	1	3
November	0	1	½	1½
December	0	2½	1	3½
Sums	4	14	12	30

TABLE V.

Sub-division of the n. w.

	W. N. W.	N. W.	N. N. W.	Sums.
January	½	2½	1	4
February	1	1	0	2
March	0	½	0	½
April	½	1½	½	2½
May	½	3½	2	6
June	1½	1½	1	4
July	2	4½	0	6½
August	0	1½	½	2
September	2	1	1	4
October	0	3½	0	3½
November	1	½	1	2½
December	2½	3½	0	6
Sums	11½	25	7	43½

By these 2 tables it appears that, of all the winds between the n. and w., those from the point of n. w. were far more frequent than those from either side of it. Of the winds between the s. and e., those from the point of s. e. were more frequent than those to the e. of that point, and rather more frequent than those to the s. of it; but the difference in the latter case was very inconsiderable. Of the winds from all quarters, those from the e. s. e. and n. n. w. were the most rare, especially the former. The numbers in the last columns of each of the last 4 tables, are the sums of the preceding columns ranging in the same horizontal lines. They ought to correspond with the numbers in columns s. w. N. E. s. e. N. W. of table 1, respectively, and serve as a check on the work in making the tables.

The general state of the winds collected from the preceding 5 tables, according to their different degrees of prevalence, is as follows:

E. S. E.	N. N. W.	W. N. W.	S. S. E.	S. E.	E.	E. N. E.	N. N. E.	S.	W.	N. W.	N.	W. S. W.	S. S. W.	N. E.	S. W.	Sum.
4	7	11½	12	14	17½	18½	20	21	24	25	25	30	34	35½	62½	361½
Days missed in the Journal.....																3½
																365

TABLE VI.

Showing the number of fair and frosty days in each half month and in the whole year.

	Fair		Fair days in whole month	Frosty days		Frosty da. in whole month
	1st half	Latt. half		1st half	Latt. half	
January	9	6	15	10	7	17
February	7	3	10	6		6
March	7	13	20	4		4
April	11	8	19			
May	8	6	14			
June	10	6	16			
July	6	8	14			
August	9	8	17			
September	7	2	9			
October	12	10	22			
November	5	6	11		1	1
December	6	13	19	5	3	8
Total fair days.....			186	Total frost ....		36

There were but 10 days in the whole year that gave any snow, viz. 3 in January, 1 in February, 5 in November, and 1 in December. The first snow on the 9th of January in the afternoon, after a rainy morning, set in with a n. n. e. wind, and was succeeded by a sharp frost for 3½ days, with the wind e. n. e. The second, which happened in the night between the 17th and 18th,

came likewise after rain, and was succeeded by a frost of  $4\frac{1}{2}$  days, wind shifting between N. W. and S. E. The last snow in January, on the 24th, fell with a S. W. wind, which set in the day before. It was followed by a moderate frost of one day, though the wind continued in the S. W. The snow on the 1st of February came with a S. W. during a sharp frost. The wind was in the N. E. before the snow, and returned to the same point the next morning; the frost sharper than before the snow. The snows in the latter part of November were generally accompanied with rain, and did not bring actual frost. The snow on the 9th of December came after 2 days frost, which it seems to have put an end to. For though it froze in the evening after the snow, the frost was much less severe than the preceding night, and a thaw came with rain, wind N. E. the next day.

There were only 2 thunder storms this year, viz. August 27, 2 p. m. barometer 29.64 inches, thermometer  $63^{\circ}$ , wind N. W. September 24, 9 p. m. barometer 29.42 inches, thermometer at 2 p. m.  $64^{\circ}$ .

TABLE VII.

For trial of the influence of the winds on the barometer.

	S. W.	W. S. W.	S. S. W.	N. E.	E. N. E.	N. N. E.	S. E.	E. S. E.	S. S. E.	N. W.	W. N. W.	N. N. W.	N.	S.	E.	W.
Jan.	29.37	29.61	29.60	29.34	29.64	29.58	29.57	29.72		29.66	29.895	29.675	29.72		29.80	29.67
Feb.	29.63	29.81	29.70	30.30		30.41				29.73	29.615		29.69	29.86	29.35	29.90
Mar.	29.39	29.79	29.89	29.945	29.87	30.20	29.52	29.83	29.52	29.615			30.14	29.87	29.90	29.75
April	29.77	30.07	29.61	29.95	30.21	29.80	30.00		29.69	29.80	29.58	29.90	29.82	29.475	29.53	29.80
May	29.91	29.46	29.45	29.96	29.86				29.71	29.81	29.97	30.04	29.98	29.34	29.925	
June	29.91	29.92	29.80	30.21		29.75	29.92		29.95	30.00	29.69	29.87	29.60	29.83	29.84	30.09
July	29.97	29.97	29.98	29.92		30.33		29.78	29.97	30.07	30.085		30.26	30.25		29.94
Aug.	29.97	29.85	29.80	29.95	30.00	30.17	30.05	30.07	29.535	29.900		30.12	30.13	29.74		29.98
Sept.	29.89	29.80	29.70	29.97	29.82	29.94	29.51		29.56	29.925	29.82	29.94	29.85	29.64	29.775	29.76
Oct.	29.98	30.28	30.225	30.32	29.865	30.23	30.15	29.54	30.18	29.98			30.19	30.22	29.95	30.27
Nov.	29.74	29.91	29.92	29.79	29.73	29.53	29.84		29.86	29.81	29.74	29.90	29.93	29.61	29.54	
Dec.	30.07	29.795	29.71	30.15	29.82	30.38	29.62		29.20	30.21	30.31		30.38	29.64	29.90	30.51
Means	29.76	29.92	29.80	30.02	29.82	30.01	29.80	29.81	29.70	29.94	29.93	29.92	29.99	29.80	29.80	30.02

It is an old observation, that a N. E. wind in this country generally makes the barometer rise. This naturally leads to an inquiry, whether there be any general connection of the rise and fall of the barometer with the setting of the wind. On comparing the general account of the barometer for the year 1774, as stated at the end of the meteorological journal, with the journal at large, I found that, in 7 months out of the 12, the greatest height of the barometer was accompanied with a north-easterly wind; and in 8 months out of the 12, the least height of the barometer was accompanied with a S. W. This incited me to take the trouble of making out the preceding table, which shows the mean height of the barometer which accompanied each wind in every month, and for the whole year. And it appears, that though the barometer may be almost at any height with any

wind, yet the mean height was greater, in the course of the last year, with the winds which set from that semicircle of the compass, which is intercepted between the points of w. s. w. inclusive, and e. n. e. exclusive, going round by the w. and n. than with the winds which set from the opposite semicircle intercepted between the e. n. e. inclusive, and w. s. w. exclusive, going round by e. and s. In the former semicircle the w. and n. e. give the greatest mean height, and in the latter the s. s. e. and s. w. give the least.\*

TABLE VIII.

For trial of the moon's influence.

	Last qr.		New.		First qr.		Full.		
	d.	h.	d.	h.	d.	h.	d.	h.	
January	5	6	11	21	19	3	27	7	$\begin{array}{cccccccc} \circ & + & \circ & + & \circ & + & \circ & + & \circ \\ 6 & 7 & 9 & 10 & 14 & 18 & 23 & 25 & 26 & 30 \\ + & - & & & & & & & & \\ & & \circ & & \circ & & & & & \\ & & & & & & & & & \end{array}$
February	3	15	10	9	18	0	25	23	$\begin{array}{cccccccc} 4 & 7 & 8 & 10 & 13 & 15 & 17 & 20 \\ & & & & & & & \\ & & & & & & & \end{array}$
March	4	22	11	22	19	20	27	11	$\begin{array}{cccc} \circ & & & \\ 10 & & & \\ & \circ & & \circ \end{array}$
April	3	5	10	12	18	15	25	22	$\begin{array}{cccc} 4 & 8 & 28 & 29 \\ & \circ & & \end{array}$
May	2	12	10	3	18	7	25	5	$\begin{array}{ccc} 5 & 7 & 23 \\ & & \text{Last qr.} \\ & & 31 & 20. \end{array}$
June		New.		First qr.		Full.		Last qr.	$\begin{array}{cccccccc} \circ & & & & & & & \circ \\ 6 & 17 & 18 & 20 & 22 & 28 & 30 \\ & \circ & & & & & \circ \end{array}$
July	8	9	16	5	22	19	29	20	$\begin{array}{ccccccc} 5 & 15 & 20 & 22 & 27 & 31 \\ & & & & & \end{array}$
August	7	0	14	12	21	3	28	12	$\begin{array}{ccccccc} 4 & 6 & 11 & 15 & 17 & 26 \\ & & & & & \end{array}$
September	5	14	12	17	19	13	27	7	$\begin{array}{cccccccc} \circ & & \circ & & \circ & & \circ & & \circ \\ 1 & 5 & 7 & 11 & 13 & 14 & 17 & 20 & 22 \\ & \circ & & & & & & & \end{array}$
October	5	3	12	0	19	2	27	3	$\begin{array}{ccc} 3 & 23 & 24 & 29 \\ & & & \end{array}$
November	3	15	10	7	17	18	25	23	$\begin{array}{ccccccc} \circ & & \circ & & \circ & & \circ \\ 2 & 6 & 7 & 18 & 21 & 26 \\ & & & & & \end{array}$
December	3	2	9	17	17	12	25	17	$\begin{array}{ccccccc} 2 & 5 & 11 & 14 & 15 \\ & & & & \end{array}$

\* It is to be noted, that the means of the whole year, stated in the lowermost horizontal row, are not found by collecting the means of all the months into one sum, and dividing by the number of months (for this method would always be fallacious, except each wind had blown for the same number of days in all the different months); but by adding together the heights attending each wind day by day, and dividing the sum by the number of days each wind blew in the whole year.—Orig.

This table exhibits a comparison of the actual changes of the weather from fair to foul, with the aspects of the moon; and needs no other explanation than an interpretation of the characters in the last column.

— frost	} Any one of these marks placed over a number signifies, that the weather indicated by that mark continued from the day of the month denoted by the number underneath to the day denoted by the next following number, bearing some other mark over it. Thus, in the month of July, rainy weather set in on the 5th, and lasted till the 15th; from the 15th to the 20th it was fine; when it changed again, and continued rainy till the 22d; then it was fine to the 27th, and rainy again till the 31st.
+ thaw	
☉ fair	
~ rainy	
~ stormy	
* snow	

Such tables of comparison, made yearly for a succession of years, would in the end decide with certainty for or against the popular persuasion of the moon's influence on the changes of our weather; which has some how or other gained credit even among the learned, without that strict empiric examination, which a notion in itself so improbable, so destitute of all foundation in physical theory, so little supported by any plausible analogy, ought to undergo. The vulgar doctrine about this influence is, that it is exerted at the syzygies and quadratures, and for 3 days before and after each of those epochs. There are 24 days therefore in each synodic month, over which the moon at this rate is supposed to preside; and as the whole consists but of 29 days 12 $\frac{1}{2}$  hours, only 5 $\frac{1}{2}$  days are exempt from her pretended dominion. Hence, though the changes of the weather should happen to have no connection whatever with the moon's aspects, though the fact should be, that they take place at all times of the moon indifferently, and are distributed in an equal proportion through the whole synodic month, yet any one who shall predict, that a change shall happen on some one of the 24 days assigned, rather than on any one of the remaining 5 $\frac{1}{2}$ , will always have the chances 24 to 5 $\frac{1}{2}$  in his favour. Merely because more changes will fall within the greater time, and, on an average, as many more in proportion as the time is greater. It is evident therefore, that this is a matter in which men may easily deceive themselves, especially in so unsettled a climate as that of this island; and the advocates for lunar influence are not to imagine they have fact on their side, unless it should appear, from such tables as these carefully kept for a long course of years, that the changes happening on the days, which they hold to be subject to the moon, are more than those which happen on the exempted days, in a much greater proportion than that of 24 to 5 $\frac{1}{2}$ .

The antiquity of the opinion may perhaps be alleged in its favour; and it may seem an answer to the objection taken from the instability of the weather of this part of the world, that it had its origin in more settled climates. We find it, it must be confessed, in the earliest Greek writers, who probably had it with

the rest of their physics from the East. And to this circumstance, I am persuaded, the opinion owes the credit it has met with among men of learning. But whatever general assertions may be found in some writers, concerning celestial influences in general, and the moon's in particular, as being of all the heavenly bodies the nearest to the earth, the writers who treat of the signs of the weather practically, for the information of husbandmen and mariners, derive their prognostics from circumstances, which neither argue any real influence of the moon as a cause, nor any belief of such an influence; but are merely indications of the state of the air at the time of observation: namely, the shape of the horns, the degree and colour of the light, and the number and quality of the luminous circles which sometimes surround the moon, and the circumstances attending their disappearance.\* It is true, that each of these prognostics is expressly confined, by the early writers, to a particular time of the moon's age.† But not, as I conceive, on account of any particular influence of the moon in this or that aspect; but merely because the prognostics, that she affords at one age, are such in themselves as she cannot afford at another. For instance, the bluntness of the horns in the new moon is a sign of approaching rain, because it indicates a turbid state of the atmosphere; for if the air were clear and dry, the horns should appear sharp and pointed, that being then their natural shape. But the bluntness of the horns is no sign of change after the dichotomy; because then the horns will appear blunt in all states of the air, the elliptic arc on the deficient side of the moon presenting its concavity to the circular limb, and forming with it an obtuse angle. Again, the degree of the moon's light on the 4th day furnished a prognostic. It ought then to be strong enough, if the air was clear, for terrestrial objects to cast a shadow.‡ If their shadows were not discernible, it was a sign that the air was impure, and bad weather was to be expected. But this prognostic did not take place before the 4th day, because the light of the moon was yet too weak for shadows to be formed in the purest state of the air. It did not take place after the 4th day; because the enlightened part was so much increased, that shadows would be

\* See the *Διοσημικα* of Aratus and the Scholia of Theon.—Orig.

† Σήματα δ' ἔτ' ἂν πᾶσι ἐν ἡμασι πάντα τίτυκται.

Ἄλλ' ὅσα μιν τρίατ' τετρατάτ' ἐκ πύλλῃαι,  
Μίσφα διχομμένης· διχάδας γέ μιν ἄχρη ἐπ' αὐτὴν  
Σημαίνει διχομνη· ἅταρ πάλιν ἐκ διχομνῶν  
Ἐς διχάδας φθιμένην ἔχεται δὲ οἱ αὐτὰ τετράς  
Μηδὲς ἀποχομνῶν. *Αρατ. Διοσημικα.*—Orig.

‡ — ὅτε πρώτη ἀποκίδνεται αὐτὸν αὐγῇ

Ὅσσοι ἐπισκιάων ἐπὶ τέτρατον ἡμῶν ἴδου. *Αρατ. Διοσημικα.*

Τεταρταία γνομένη ἢ σιλήνῃ ἄρχεται δύνασθαι σκιάζειν ἐν τῷ φωτὶ αὐτῆς· τεταταία γὰρ ἔδύναται διὰ τὴν περικειμένην τῷ φωτὶ ἀδράνεια

Theon in locum.—Orig.

formed in any state of the air, if the moon was not actually hidden by a cloud, or obscured by sensible mists. The prognostics furnished by the new moon served only till the dichotomy, and those of the dichotomy till the full moon, and so on; not because a new and distinct influence was exerted in each new aspect, but because each new aspect furnished a new set of signs, of a different kind. That this is a true representation of the most ancient lunar prognostics, appears from hence; that others of a similar kind were derived from the sun and the fixed stars, particularly the Præsepe and Aselli in Cancer, and the bright star in the Altar; and it is remarkable, that Aratus says, the prognostics taken from the sun are the most certain of all.\* The vulgar soon began to consider those things as causes, which had been proposed to them only as signs. The manifest effect of the moon on the ocean, while the mechanical cause of it was totally unknown, was interpreted as an argument of her influence over all terrestrial things; and these notions were so consistent with that visionary philosophy, which assigned distinct places to corruption, change, and passivity, on the one side, and the active governing powers of nature on the other, and made the orb of the moon the boundary between the two, that they who should have been its opponents, ranged themselves on the side of popular prejudice. And the uncertain conclusions of an ill-conducted analogy, and a false metaphysic, were mixed with the few simple precepts derived from observation, which probably made the whole of the science of prognostication in its earliest and purest state. Hence both Theophrastus and Aratus teach us to remark the position of the moon's horns, and take conjectures of approaching fair weather or tempest, according as they appear, at different times of the moon's age, erect, reclined, or prone: not knowing that the position of the line joining the moon's cusps, with respect to the horizon, depends merely on the mutual approach, or recess, of the pole of a great circle drawn through the centres of the sun and moon, and the pole of the horizon, in the course of the diurnal revolution. And so great a man as Varro, as he is quoted by Pliny, was not ashamed to give this childish rule, for predicting the weather, for a whole month to come, from appearances at the new moon. "If the upper horn be obscure, the decline of the moon will bring rain. If the lower horn, the rain will happen before the full. At the time of the full moon, if the blackness be in the middle."† After this one cannot be surprized, that the poet Virgil should make the prognostics of the 4th day decisive for the whole lunation:

\* *Ἡελίῳ καὶ μᾶλλον τοιότατα σημεῖα καίται. Διοσημεία.*—Orig.

† *Apud Varronem ita est.*——Nascens Luna si cornu superiore obatro surget, pluvias decrescens dabit: si inferiore, ante plenilunium: si in mediâ nigritia illa fuerit, imbrem in plenilunio. *Plin. Nat. Hist. lib. xviii. cap. 35.*—Orig.



Sin ortu quarto, namque is certissimus auctor,  
 Pura neque obtusis per coelum cornibus ibit,  
 Totus et ille dies, et qui nascentur ab illo,  
 Exactum ad mensem pluvia ventisque carebunt.

Georgic. lib. 1, lin. 143.

But in this he contradicts Aratus, whose authority in general he follows implicitly. With Aratus, the signs of the new moon extend only to the first quarter.

The ancients ascribed an influence to the constellations and fixed stars as well as to the sun and moon; and there seems to have been much the same foundation for one as the other. In the *parapegmata* or calendars, introduced in Greece, as we learn from Theon,\* by the astronomer Meton, and renewed either annually, or as I rather conjecture, at the expiration of every 19 year period, the heliacal risings and settings of different stars were marked as bringing in different sorts of weather. The truth is, the earliest astronomers imagined, that the weather was governed by the sun, and that its varieties were every where owing to the different degrees of the sun's heat in the different seasons. They had therefore taken great pains to collect, by a long series of observations, the weather that usually prevailed in this or that particular place during the sun's passage through every degree of every sign. Upon these observations, not upon any whimsical theory of celestial influences, the predictions in the calendars were founded. It seemed reasonable to announce, as the weather of each part of the year, what had been found to be then most frequent. And while the civil reckonings of time were so different among the different Greek states, and so rudely digested in all, the heliacal risings and settings of the stars were the only certain and obvious marks, the compilers of those popular directories could hit upon, of the sun's return to the different parts of the zodiac.† Hence they proposed them to people as signals of the weather to be expected. The form of the year being now the same in all parts of Europe, and pretty accurately adjusted to the motions of the heavenly bodies, and the heliacal risings and settings of the stars, from the different manner of life of our country people, not falling so much under popular observation with us, as they did among the Greeks, they are not marked as prognostics in our modern almanacks: and this I take to be the reason, that though the moon hath maintained her reputation among us, the influence of the fixed stars is sunk, as it well deserves, in utter oblivion. On the whole I do not deny, that the observant husbandman will find a variety of useful prognostics in the appearances of the moon, and the heavenly bodies in general; but they will be prognostics of no other kind, and for no other

\* Scholia in Aratum.—Orig.

† Geminus. *Εισαγωγή εις τὰ φαινόμενα*. c. 14.—Orig.

reason (though perhaps less fallible) than the sputtering of the oil in the industrious maiden's lamp, or the excrescences which gather round the wick.\* They will be symptoms destitute of all efficient powers. They will show the present state of the air, as that on which they depend, not as that which they govern, and may furnish probable conjectures for 2 or 3 days to come. To what I have already advanced in support of this opinion, I shall only add the last lines of the *Διοσημεία* of Aratus. They speak the sentiments of the earliest ages most decisively, as they show how little the doctrine of the influence of lunar aspect had gained ground, even in his days, among practical writers. That elegant versifier, there is little room to doubt, delivers the practical maxims of his time, just as he received them. He was too little of a poet to disguise the truth with ornamental fiction, and too little of a philosopher to adulterate it with hypothesis.

Τῶν μηδὲν κατόκησο, καλὸν δ' ἐπὶ σήματι σῆμα  
 Σχίπτεσθαι, μᾶλλον δὲ δυοῖν εἰς ταυτὸν ἰόντων  
 Ἐλπωρὴ τελεῖται· τριτάτῳ δὲ κε θαρσυσίας.  
 Αἰεὶ δ' ἂν περιάντες ἀριθμοῖς ἑνίαυτῇ  
 Σήματα, συμβάλλων εἴπη καὶ ἐπ' ἀσέρι τοίῃ  
 Ὅπως ἀντέλλοντι κατέρχεται, ἢ κατιόντι,  
 Ὅπποῖον καὶ σῆμα λέγοι· μάλα δ' ἄρκιον εἶη  
 Φράζεσθαι φθίνοντος, ἐφισταμένοιο τε μηνὸς  
 Τετράδας ἀμφοτέρως. αἱ γὰρ τ' ἄμυδις συνιόντων  
 Μηνῶν πειρατ' ἔχουσιν, ὅτε σφαλερώτατος αἰθῆρ  
 Ὅκτῳ νυξὶ πέλει, χῆται χαροποῖο σελήνης.  
 Τῶν ἄμυδις πάντων ἐσκεμμένος εἰς ἑνιαυτὸν,  
 Οὐδέποτε σχεδίως κεν ἐπ' αἰθέρι τευμήραιο.

Which I render thus: "Neglect none of these prognostics [none, he means, of the great variety he hath enumerated, taken from the heavens, from animals, plants, terrestrial objects, &c.], it is a good thing to combine the observation of one prognostic with another. If 2 agree, there is the greater likelihood of the event, and a 3d makes it certain. Whatever you do, register [*ἀριθμοῖς*] the prognostics of the current year, carefully noting what the prognostic says [*ὅπποῖον καὶ σῆμα λέγοι*; that is, what the event shows it to be a sign of], if such

\* Ne nocturna quidem carpentes pensa puellæ  
 Nescivere hiemen: testè cum ardente viderent  
 Scintillare oleum, et putres concreacere fungos.

Georgic. lib. i. lin. 390.

Ἡ λύχνος μόνη τις ἂν εἴηται περὶ μόχθου,  
 Νύκτα κατὰ σκοτίαν, μὲν ἢ ἀπὸ χείματος ὄρη  
 Δόχον ἂν ἴσῃς μὲν τε φῶς κατὰ κόσμον ἰσῇ,  
 Ἄλλοτε δ' ἄισσασιν ἀπὸ φλόγης, ὅτε κῆφαι  
 Πομφόλογος &c. Αριστ. Διοσημ.—Orig.

a sort of morning\* come on with the rising or setting of any particular star. And it will be of the highest importance to attend particularly to the 2 quaternions of the expiring and the incipient month† [that is to the last 4 days of the month going out, and the first 4 of that which is setting in], for they comprize the extremities of the 2 months, where they meet: and the weather [or the state of the air] is then particularly uncertain [difficult to guess at] for 8 nights, for want of the silver-coloured moon. If you attend to all these put together, all through the year, you will never form a random guess about the weather." The uncertainty of the weather for these 8 nights cannot be an uncertainty of the effect depending on the moon's aspect; but it is an uncertainty of foreknowledge, the poet speaks of, for want of the moon as an index. For though the word *σφαλερώτατος* by itself would be ambiguous, as it might be taken either in the sense of *δυσόχαστος* or *εὐμετάβλητος*, the words *χῆται χαροτοῖο σελήνης* are decisive for the first interpretation. The moon exists during these 8 months as at other times. There is no want of her therefore as a physical agent: the only want there can be, is the want of her appearance. It would be unpardonable not to mention, that so great an authority as that of Theophrastus is against the side of the question to which I incline. The doctrine of the influence of lunar aspect is expressly asserted in his Treatise on the Signs of Rain and Wind. He says, that the new moon is generally a time of bad weather, because the light of the moon is wanting;‡ and that the changes of the weather generally

\* Such a sort of morning.—That is, a morning marked with such or such appearances. So I understand *τοῖς ἡμέραις*. The spirit of the precept seems to be, that the heliacal risings of the stars are to be attended to, in conjunction with the particular appearances attending the dawn or sun-rise. The heliacal risings show the season, or general constitution of the time of the year; the particular appearances of the morning indicate the minute circumstances of the weather for 2 or 3 days to come. Thus the heliacal rising of Arcturus was a sign, in all the ancient paraepgmata, that the stormy season was at hand, and bad weather of various sorts, rain, thunder, high wind, was to be expected; but what the particular weather would be for a day or two to come, whether it would be only windy, or wet, with thunder or without, from what quarter the bad weather would come, all this would be pre-signified by the particular appearances of the morning. Perhaps the same appearance may be subject to some variety of interpretation at different seasons of the year, and in different places. In this, experience and observation will be the only sure guides. And for this reason Aratus advises his scholar, not only to attend to the general rules laid down for him, but to keep a journal for himself, and make his own conclusions.—Orig.

† And it will be of the highest importance to attend to, &c. *μάλιστα δ' ἄριστον εἰς φράζειν*. I have sometimes thought these words might be rendered thus: "This will be of great importance [that is, this joint observation of the general indications of season and of particular prognostics will be of great importance] in order to form a conjecture about the two quaternions, &c." This interpretation would make the most connected meaning for the whole passage; but I do not recollect, nor can I find on the strictest search, any instance, wherein the verb *φράζειν* is used in the sense of conjecturing, or forming a judgment or opinion about.—Orig.

‡ *Διὸ καὶ αἱ συνόδοι τῶν μηνῶν χειμῆριός ἐστιν, ὅτι ἀπολείπει τὸ φῶς τῆς σελήνης*, &c. Theophrast. de signis Pluv. p. 417. Edit. Heins.—Orig.

fall on the syzygies or quadratures. But this seems to have been merely an opinion founded on an imaginary analogy between the epochs of syzygy and quadrature in the months, and the equinoctial and tropical epochs in the year. For the moon, he says, is, as it were, the sun of the night. Theophrastus, though a diligent observer of nature, was deep in the theory of that school, of which he was himself one of the brightest ornaments: and his testimony, with respect to the matter of fact, hath not, like Aratus's, a credibility founded on the mediocrity of his genius.

In the table, p. 620, the changes which fell on the syzygies and quadratures, or on any one of Pliny's critical days of the moon's age (which are the 3d, 7th, 11th, 15th, 19th, 23d, 27th), are distinguished from the rest by a larger character.\* And out of 69 changes registered in this table, 32 claim that distinction. Which is rather a larger proportion of the whole number, than is due to the time made up of all the days of syzygy and quadrature, in the whole year, together with Pliny's critical days, thrown into one sum. For since there were 365 days in the year, and the days of syzygy and quadrature, with Pliny's critical days, amount to 113, out of 69 changes in the whole year, 22 are as many as belong to these particular days, on a proportional distribution. But in the preceding table, there are many alterations marked as changes, when it appears that the weather returned to what it had been before the time of change, within the space of 24 hours after it. Now if we reject all these on both sides of the question (which I think is the fair way of reckoning, for sudden alterations, of so short a duration, are rather to be called irregularities than changes of weather), we shall find but 46 changes in all, from one settled state to another, of which only 20 fell on the days of syzygies, quadrature, or Pliny's days, which is still more than the just proportion.

But again. Pliny's 8 critical days were probably intended for the 4 days of syzygy and quadrature, and the 4 of octagonal aspect.† For if the time of the conjunction be rightly assumed, the mean quadratures, and the mean opposition, and the mean octagonal aspect, will always fall either on one of Pliny's days, or on the day next to it. The deviation, I suspect, was intentional, and for the sake of the odd numbers. Thus the 4th, 8th, and 12th days of the moon should have been critical, instead of the 3d, 7th, and 11th, if the mean motions of the moon had been the single thing attended to. But Pliny, or whoever was the first author of the rule he gives us, chose the latter as containing, besides much of the lunar influence, all the magic virtue of imparity,

\* Sunt et ipsius Lunæ octo articuli quoties in angulos solis incidit, plerisque inter eos tantum observantibus præsagia ejus, hoc est tertia, septima, undecima, decima quinta, decima nona, vigesima tertia, vigesima septima, et interlunium. Plin. Nat. Hist. lib. 18, c. 35.—Orig.

† The words, Quoties in angulos solis incidit, imply this.—Orig.

of which the others, taking their numerical denomination from even numbers, are totally destitute. Among the numerous believers in the moon of our days, few, I suppose, retain any confidence in the physical powers of the odd numbers. They may imagine therefore, that the apparent inconsistency of Pliny's rule with the truth of things, may be owing to his superstition about the odd numbers, which led him wilfully to deviate from the mean epochs, little apprized (for the Romans never were astronomers) how much they sometimes differ from the true ones, on account of the great and various inequalities of the moon's motions, and how very widely his arbitrary arrangement would in consequence often differ from the times it was intended nearly to represent.

Instead of Pliny's critical days, I shall now therefore examine the days for which I imagine they were substituted; those I mean of true syzygy, true quadrature, and true octagonal aspect. The following table distinguishes the changes of weather which fell on these days. There were only 22 such, out of all the 69; which is scarcely 4 more than their even proportion. And rejecting, as before, on both sides, the alterations of weather which were reversed within the space of 24 hours, there remain, out of 46 changes in all, only 10 on the days of lunar influence, which are 2 less than belong to them on the even chance; for the days of syzygy, quadrature, and octagonal aspect, in the whole year are 98; and  $365 : 98 = 46 : 12\frac{1}{2}$  very nearly.

TABLE IX.

Jan.	6 7 9 10 14 18 23 25 26 30	10	3
Feb.	4 7 8 10 13 15 17 20	8	3
March	10	1	0
April	4 8 28 29	4	1
May	5 7 23	3	0
June	6 17 18 20 22 28 30	7	3
July	5 15 20 22 27 31	6	1
Aug.	4 6 11 15 17 26	6	2
Sept.	1 5 7 11 13 14 17 20 22	9	4
Oct.	3 23 24 29	4	1
Nov.	2 6 7 18 21 26	6	3
Dec.	2 5 11 14 15	5	1

69 22

I have added in this table 2 columns, showing the number of changes in each month, and the number out of each agreeing with the moon. I shall only add, that no conclusion must be drawn from the observations of a single year.

*XVII. Extract of a Meteorological Journal for the Year 1774, kept at Bristol.*  
*By Samuel Farr, M.D. p. 194.*

Months.	Barometer.			Vici.	Rain.
	Highest.	Lowest.	Mean.		
Jan.	30.1	28.8	29.5	1 1-2	4.951
Feb.	30.4	29.2	29.7	0 9-1	5.549
March	30.2	29.1	29.7	0 9-4	5.297
April	30.1	29.3	29.7	0 8-5	2.349
May	30.1	29.3	29.9	0 7-4	2.955
June	30.2	29.4	29.7	0 6-3	2.602
July	30.2	29.7	29.8	0 4-1	2.972
Aug.	30.2	29.4	29.8	0 5-2	2.999
Sept.	30.1	29.0	29.6	0 7-2	7.035
Oct.	30.5	29.3	30.0	0 8-2	1.927
Nov.	30.2	29.2	29.7	0 6-1	1.683
Dec.	30.6	29.0	29.7½	0 7-2	2.047

Mean of the whole was 29.74

42.366

The barometer was placed 17 yards above the level of the river Avon, which runs very near the house. By vicissitude is meant the greatest rise or fall of the quicksilver in the smallest number of days.

Dr. Farr had also given the mean heights of the thermometer within doors for every month in the year. But these are omitted, because observations

of the thermometer in the house are of no importance, unless accompanied with corresponding ones of an instrument kept in the shade in the open air. The air of a room, though kept without a fire, and so situated as never to see the sun, alters its degree of heat or cold so much more slowly than the external air, that no judgment can be formed of the temperature of the one from that of the other; except after a continuance of weather of the same kind for a long time together, their mutual relation is vague and undetermined. Dr. Farr, likewise sent a particular account of the winds and changes of the weather for every day of the year; from which I have composed the two following tables.

S. HORSLEY.

*An Abridged Table of the Winds for Bristol, for the Year 1774.*

	N.	S.	E.	W.	N.W.	S.E.	N.E.	S.W.	Da.	Number of frosty days.
January	3½	½	6	3	1½	2	7	7½	31	10
February	4	1½	½	1	3½	3	5½	11½	27	7
March	1½	1½	4½	½	3½	5½	11	4	31	7
April	½	2	½	0	8	4½	5	8½	29	
May	1½	1½	2	0	2	2	14½	8½	31	
June	1	2½	2	½	4	2	2	16½	30	
July	1	1	0	2	6½	2	0	17½	30	
August	0	½	1½	0	1	4	6½	17½	31	
September	½	½	0	½	4	10	7½	7	30	
October	0	1	2	½	3½	6	5½	12½	31	
November	1	½	0	0	4	5	13	6	30	
December	0	0	3	0	½	8	13½	6	31	
	9	13	22	8	42	53	92	123		

3 days in the year are omitted in Dr. Farr's account, viz. Feb. 7, April 29, and July 12.

Frost at times.  
Frosty nights.  
18

42

Thunder, February 16, 23, 24, s. w.—March 8, 20, E. 28, E. and w. 30, E. and N. E.—April 27, with hail storm, s. w. and N. w.—May 1, 4, N. E. 9, 10, E. 24, s. w. and s. E.—June 25, s. w. and s.—July 10, s. w. 26, N. and N. w.—September 4, s. E. and N. E. 6, N. w. 12, s. w. and s. E.

Table for Trial of the Moon's Influence at Bristol, for the Year 1774.

	Last qr.		New.		First qr.		Full.				
	d.	h.	d.	h.	d.	h.	d.	h.			
January	5	6	11	21	19	3	27	7	1 5 10 13 19 22 30	7	2
February	3	15	10	9	18	0	25	23	7	1	1
March	4	22	11	22	19	20	27	11	6 10 17 20 30	5	1
April	3	5	10	12	18	15	25	22	6 10 13	3	1
May	2	12	10	3	18	7	25	5	1 10 19	3	1
							Last qr.				
							31	20			
June	New.		First qr.		Full.		Last qr.		1 6 13	3	1
	8	18	16	19	23	12	30	7			
July	8	9	16	5	22	19	29	20	1 10 14 17 19 26	6	2
August	7	0	14	12	21	3	28	12	1 3 7 25	4	3
September	5	14	12	17	19	13	27	7	Only 9 fair days.		
October	5	3	12	0	19	2	27	3	6 17 25 30	4	4
November	3	15	10	7	17	18	25	23	Cloudy with rain till the 5th, hard rain, then frequent frosty nights, and gentle rain in the day-time.		
December	3	2	9	17	17	12	25	17	3 11 21	3	2

39 14

When a number appears in this table without any character over it, it is to be understood, that the weather was quite unsettled from that day to the next bearing a mark; and when 2 or more marks are found over the same number, all the different kinds of weather, denoted by the several marks, took place on that day. The same is to be understood in the tables, in my paper preceding.

This table distinguishes the changes of weather which fell on the days of true syzygy, true quadrature, and true octagonal aspect. Setting aside the very changeable months of September and November, there were 39 changes in the remaining 10, 14 of which happened on the days specified; which is almost 4 more than belong to them on the even chance. Of these 14 changes, only 4 fell on the day of a new moon, and none at all on the day of the full.

S. HORSLEY.

*XVIII. Extract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1774. By Thomas Barker, Esq. p. 199.*

		Barometer.			Thermometer.						Rain.
		Highest.	Lowest.	Mean.	In the house.			Abroad.			.
					High.	Low.	Mean.	High.	Low.	Mean.	
January	Morn.	20.77	28.32	29.15	42	31½	35	43	20	29	3.308
	Aftern.				43	32	36	46	28	33½	
February	Morn.	30.05	28.49	29.25	46	33½	40	45	22	34½	1.946
	Aftern.				46½	35	41	51½	29½	41	
March	Morn.	29.81	28.56	29.30	48½	38	43	44	28½	36	2.728
	Aftern.				51	39	44½	57½	35½	46	
April	Morn.	29.77	28.72	29.24	53	44½	48	52½	32½	42	1.523
	Aftern.				54½	45½	49	62½	37½	51	
May	Morn.	29.67	28.76	29.35	55	48	51½	55½	40	46	3.142
	Aftern.				56½	49	53	69½	45	57	
June	Morn.	29.76	28.87	29.33	62	54	59	61	50	55	2.483
	Aftern.				66	55½	60	73½	56	65½	
July	Morn.	29.76	29.10	29.41	63½	57½	60	61	52	56	3.227
	Aftern.				66½	58½	62	76½	61	66	
August	Morn.	29.80	28.80	29.38	68	58	61½	64	47	55½	3.910
	Aftern.				70	60	63½	78½	59	67	
September	Morn.	29.74	28.70	29.28	65	53	56	61	40	49½	8.000
	Aftern.				68½	53½	57½	73	48½	59	
October	Morn.	30.06	28.92	29.64	56½	46	52	51	34	43½	1.156
	Aftern.				57½	46	53½	64½	42	53	
November	Morn.	29.73	28.73	29.36	52½	35½	43	49	28	37	1.530
	Aftern.				52½	36	44	55½	32	41	
December	Morn.	30.21	28.68	29.60	45½	32	39½	44½	20	33½	2.280
	Aftern.				46	32½	40	47	25	38	
Means of all .....		29.35		29.35	49.7		49.7	47.3		35.235	

*XIX. Of some Thermometrical Observations, made by Sir Robert Barker, F. R. S., at Allahabad in the East Indies, in Lat. 25° 30' N. during the Year 1767, and also during a Voyage from Madras to England, in 1774. From the original Journal by the Hon. Henry Cavendish, F. R. S. p. 202.*

The greatest part of the observations at Allahabad were made within doors; several were made within a tent placed under the shade of trees, some in the open air in the sun, and some in the open air in the shade; but there is no regular series of observations in any one place; nor were they made at stated times of the day. Though a thermometer kept within doors is but a very indifferent measure of the heat of any climate; yet as I have not seen any thermometrical observations made in that country, except a few during the heats of the summer, and printed in the Philos. Trans., vol. lvii, p. 218, I have set down the greatest and least heights met with in each month.

	Least.	Great.		Least.	Great.		Least.	Great.
January	58	72	May	72	101	September	78	83
February	60	84	June	81	99	October	72	87
March	62	94	July	81	90	November	52	86
April	79	96	August	80	86	December	51	64



From the 3d of May to the 4th of June inclusive, a thermometer placed within a tent, under the shade of trees, was almost every day above  $100^{\circ}$ , and several times above  $109^{\circ}$ , once at  $112^{\circ}$ . The trees under which the tent was placed, formed a very thick shade; so that probably these heights are more likely to fall short of the true heat of the open air at that time, than to exceed it. The least height he met with of the thermometer in the open air in the shade, is  $42^{\circ}$ ; which it was at twice in the month of January, at 7 A.M. The greatest heat is on June 9th, at noon, when it was at  $114^{\circ}$ , the sky cloudy; the thermometer within doors at the same time  $95^{\circ}$ , which is less than it had frequently been in the month of May; so that it seems likely, that the heat in the open air in May had frequently been above  $114^{\circ}$ . During the voyage to England, the thermometer was placed in the round-house, and was observed regularly at 8 in the morning, at noon, and at 3 in the afternoon; the winds and weather are also set down. The round-house is one of the uppermost row of cabins, and is reckoned the coolest and most airy part of the ship. From February 13 to April 7, between Madras and the southern tropic, the thermometer was constantly between  $77^{\circ}$  and  $86^{\circ}$ , and very seldom lower than  $80^{\circ}$ . From that to April 23, lat.  $34^{\circ} 12'$ , about  $15^{\circ}$  E. of the Cape of Good Hope, between  $70^{\circ}$  and  $80^{\circ}$ . Thence to May 20, at St. Helena, between  $62^{\circ}$  and  $72^{\circ}$ . Thence to August 2, in lat.  $43^{\circ} 14' N$ , between  $71^{\circ}$  and  $80^{\circ}$ ; and thence to August 15, in the British Channel, between  $62^{\circ}$  and  $70^{\circ}$ . At land it is well known that the heat is usually considerably greater in the middle of the day, than in the morning or night; but it appears from these observations, that in the open sea, there is scarcely any sensible difference; for in settled weather, the difference between the different times of the day was rarely more than  $1^{\circ}$ , oftener none at all. In unsettled weather there was frequently a difference of  $2^{\circ}$ , sometimes  $4^{\circ}$ , scarcely ever more; but then there seems no connection between this difference and the time of the day, it being as often colder in the middle of the day than in the morning or evening, as warmer. There is added a register of the thermometer, in the soldiers' barracks at Allahabad, on June 8, 1769, when from 10 in the morning to 8 in the afternoon it stood constantly above  $100^{\circ}$ , in the hottest part of the day at  $107^{\circ}$ , and during the whole night between  $99^{\circ}$  and  $98^{\circ}$ .

Sir Robert Barker gives the following account of the general state of the weather in Bengal.

The rains at Bengal generally set in between the 1st and 15th of June, and continue till the middle of October, when it remains fair till February, the wind blowing mostly from the N. E. quarter, in which month and March it is interrupted by the N. W. squalls, attended with violent gusts of wind, thunder, and lightning, with short, but excessive hard showers of rain or hail, commonly one, but rarely 2 in each day. From the middle of March to the middle of

June the weather is very hot. At Allahabad and the upper country the rains are not expected till the 20th of June, and seldom exceed the 30th, excepting in extraordinary seasons, when it has been known to keep off till the 5th of July; but such an event is usually attended with a great mortality both of men and beasts. They break up about the middle of Sept. and from this time to the beginning of Jan. it continues fair cold weather. In Jan. there are almost always a few days rain, seldom more than a week, and that gentle and pleasant, which is productive of a 2d crop, which they usually reap. The winds at Allahabad set in easterly from the beginning of the rains, and blow almost constantly from that quarter till the conclusion of the cold weather in March, when it changes more northerly, and is attended by violent north-west squalls of thunder, lightning, rain, and hail, at which time it changes to the west, blowing with violence, and a heat which frequently destroys the birds and beasts in the fields, till the rain affords a relief. The river Ganges begins to swell before the commencement of the rains, reported by the natives to proceed from the melting of the snow on the northern mountains during the heats of May and June. But the sudden rise of the waters in the Ganges, a few days after the setting in of the rains, is almost incredible; since it has been known to rise 20 feet in 48 hours; and its sudden fall is as extraordinary. In Bengal the rivers are of course affected by the rise and fall of the Ganges. Floods continue the whole time of the rains, more or less; but the greatest overflowings are generally at the beginning and the end or the breaking up of the rains, at which period it rains with the greatest violence. The waters at Allahabad, and in all the upper countries, run off into the rivers as soon as the rain has ceased, the soil being for the most part of sand, and the country intersected with small rivulets; but in Bengal, and particularly so low down as Calcutta, being of a clay soil and an extensive flat, the whole country is overflowed, forming lakes of great extent, some of them being 6 miles over. The water therefore generally remains till the sun has exhiled it, by which it becomes putrid, and renders those parts extremely unwholesome, occasioning those deadly putrid fevers, which carry off the patient in a few hours, known by the name of pucker fevers.

*XVI. A 2d Essay on the Natural History of the Sea Anemonies. By the Abbé Dicquemare. Translated from the French. p. 207.*

I was concluding my essay on the sea anemonies, says Mr. D., inserted in the 63d volume of the Philos. Trans., [page 460, of this abridged volume.] when I discovered a 4th species of that animal; and I have reason to think that I have since observed a 5th species. New observations have increased the number of my experiments: my ideas have been enlarged, my views extended; and the phenomena crowd in so fast upon me, that I dare not flatter myself with the

hopes of ever arriving at the end of this pursuit. The scarcity of high tides, the vicissitudes of seasons, and other similar impediments, make it less wonderful that a series of years should often elapse before it is possible to present the curious with any discoveries of which they might avail themselves, either by analysis, combination, or analogy, and thereby furnish general views and a chain of ideas leading to a new field of discovery, the usual effect of contemplation. I shall here communicate some of the ideas that have been suggested to me by my last experiments.

How many are the animal functions, which seem to depend on sensibility and irritability; and yet how little are these faculties understood? how ignorant are we of their cause? The nerves seem to be the chief, perhaps the only organs of sensibility in man, and the muscular fibres to be the principal seat of irritability; yet how many are the doubts entertained concerning the parts that are and are not endowed with one and the other! how false and erroneous the conclusions relating to the effects they produce, notwithstanding the many experiments made on animals, whose interior structure is the most similar to our own! It is then from accurate observations on such animals as bear the least resemblance to our species that we may hope for new discoveries. The sea anemonies are exceedingly gelatinous, and at the same time so irritable, that even light affects them, though to all appearance destitute of eyes. Might not the rapid and singular reproduction of the parts of this animal be attributed to their gelatinous texture? and if so, may we not reasonably conclude, that the reproduction of our vascular and fleshy parts in the consolidation of wounds, is in great measure owing to such a gelatinous matter; and should we not seek for means to increase or diminish the quantity of that matter as circumstances may require. If it be true, that earth and a gelatinous substance are the constituent parts of the muscular fibres of such animals as we are best acquainted with, and that only the latter are capable of irritation; doth it not follow, that the gelatinous nature of the sea anemonies is the true cause of the effect produced on them by the impression of light? and may we not conjecture, from the very gelatinous nature of these animals, and from their being affected by light on every part of their bodies, but more particularly on those that are recently cut; may we not then, from hence conjecture, that the gelatinous part of the muscular fibre is the only one capable of irritation in ourselves? Might not these animals, by a sober use of analogy, or by new experiments, lead us to a more perfect knowledge of those singular enemies to man, the tape, the hair worm, and the sea-dragon?

I continued to observe the inferior half of a purple anemony of the first species, which I had cut in two on the 12th of July 1772, and which was alive on the 8th of April 1773, the day on which I concluded my former essay: it ap-

peared to be daily recovering strength. On the 26th I found it at the bottom of the vessel. On the 1st of July it climbed up the sides almost to the surface of the water; and this it repeated on the 15th and 22d, above a year after the time it had been cut. On the 25th a crab (*cancer lanosus*, *cancer venenatus*) half dried, fell into the vessel, and after continuing in it some hours, infected and tinged the water in the same manner as if husks of walnut or pieces of soot had been thrown into it, which had such an effect on the piece of anemony, that it threw up a great quantity of its intestines. On the 30th it laid hold of the side again, but was considerably shrunk. In the beginning of September it received a 2d injury, from another piece of anemony, which having been damaged in the same manner by the former accident, suddenly putrified and infected the water: more of the intestines were now discharged; and this last accident, added to the former one, affected the creature to such a degree, that it wasted gradually till the 8th of October, when it was totally dissolved. The sea anemonies are undoubtedly susceptible of irritation to a very great degree; but is all that has been described to be considered as the mere effect of irritability? Allowing that to be the case, will it not follow, that we are more in the dark concerning that faculty than is generally thought? It is usual to ascribe to it the palpitation that is perceived in the flesh of oxen, when cut from the animal, in the severed pieces and hearts of some reptiles, as the sloth, and other involuntary spasmodic motions; but is it possible that determinate motions, that actions which seem to imply will, such as clinging, &c. which in our experiment were continued for the space of 15 months, and, but for an accident, might probably have been carried on much longer, should arise from mere irritability, without any other cause? The upper part of another sea anemony, of which the inferior was become a perfect animal, lived 6 months after its being cut, and seemed to feed by suction on pieces of muscle I put in its way.

Sea anemonies, cut diametrically and perpendicularly, were not essentially hurt by that operation; which might be expected to disorder more than any the whole animal economy, and to be particularly injurious to the basis of this animal, which is its most essential part, and in some species is exceedingly tender. The two sides soon came together, but were some time in contact before they connected. The junction however was at last so perfect, that no visible scar remained on the robe, the continuity of the little blue edge was not in the least interrupted, and the mouth was perfectly restored. These semi-anemonies have long since acquired the appearance of the perfect animal, and perform all its functions, such as moving from place to place, swallowing, &c. This leads to the reflection, that if, as has been asserted, the power of locomotion in these animals depends on a certain combination of straight and circular tubes, it is not requisite, in order to exert it, that the continuity of these

tubes be uninterrupted, since half an anemony newly cut changes its place with as much ease as a whole one. It will no doubt appear a curious inquiry, whether these semi-anemonies, after becoming in a manner whole ones, are capable of propagating their species. To this I can only answer at present, that I have not yet seen the generation of anemonies, except in the sea, or in animals newly taken out of it. It must further be observed, that these anemonies, perfect as they seem to be, may perhaps have only half the number of limbs of the whole ones, of which they made a part: so that the whole wonder comes to this, that the severed halves of an animal should recover, and each taking the appearance of an entire individual, continue to live as if they were such. And such in fact I believe they are; but this I have not yet been able to ascertain, as the anemonies of this species have not all the same number of limbs, as it is always very difficult to count them, and as all those on which I have hitherto made the experiment had a great number of them. However, as no manner of difference appears, I am inclined to suspect that new limbs shoot out between the old ones.

After having observed these animals during several years, both in the sea and in my study, it will no doubt be expected, that I should now give a particular account of their manner of propagation; but here I can only confess my ignorance, having never been able to get at the knowledge of any one circumstance relating to it: which makes me suspect, that these animals propagate without any communication of individuals. What I would here suppose, is by no means unexampled. Among the aphides, for instance, whose mode of propagation deserves to be further examined, though the sexual parts have been discovered, individuals nevertheless are found, which, though deprived of all communication one with another from the very moment they are brought forth, yet produce an offspring, which being likewise denied all intercourse, still propagates; and so on, through a great number of generations, which succeed each other very rapidly. The muscle also is thought to be an animal of the same nature.

The anemonies of the 2d species are not only less obvious to our observation, but it is with difficulty they are preserved in any degree of perfection. They cannot be taken out of the sand without depriving them of their natural position. Common mixed sand kills them in a few days; and that which is purified affords them no longer the slime, the small insects, or other necessary sustenance, which we cannot possibly divine. In plucking them from their native soil, their bases generally suffer, and the wounds in that part are frequently mortal. One of the safest expedients is to gather with them the pebbles to which they adhere; or what is still preferable, to observe them in their natural element the sea. It is there that, without the least hostile appearance, they are seen to make an in-

credible havoc. I have seen an anemony of a moderate size swallow a smelt at least 6 inches long. The limbs of this species, which are much thicker than those of any other, being clipped, new ones shoot out as in former cases. The progress of this reproduction, which is effected in a few days, is scarcely perceptible; and it is so perfect, that no protuberant rim or visible scar remains. Neither the colour, the size, nor the form are any ways altered. This anemony is able to creep when deprived of its limbs; which seems to prove, that the communication, which is thought to exist between the limbs and the hollow muscles, may be interrupted, without sensibly restraining the animal's locomotive powers. Those limbs, it is true, enable the animal to crawl when turned on its back; but do by no means serve as legs for walking steadily, as hath been erroneously asserted, and misrepresented in ill-drawn figures. I made large incisions on several anemonies in the sea, which healed in a very short time; but I always took care not to injure the basis, as any considerable wound, and especially the least rent, on that part of this species, proves often mortal. I do not mean to question the possibility of what hath been repeatedly said of an anemony, which not being able to void a muscle it had swallowed, forced it out through a rent it made with the muscle itself at its basis, and that this rent was soon after perfectly cicatrized. But a love of the marvellous too plainly appears throughout the whole narration, and the inferences drawn from the fact give room to suspect, that little attention had been paid to the concomitant circumstances. Wounds of this nature often occasion a disorder in the interior part of the anemony, the progress of which soon brings on its total dissolution. Of all the kinds of sea anemonies, I should prefer this for the table: being boiled some time in sea water, they acquire a firm and palatable consistence, and may then be eaten with any kind of sauce. They are of an inviting appearance, of a light shivering texture, and of a soft white and reddish hue. Their smell is not unlike that of a warm crab or lobster. I have seen some of the young of this species, but have not been able to make any discovery concerning their mode of propagation.

A detail of my former observations and experiments would be here a useless repetition: I shall therefore only observe, that the semi-anemonies of the 3d species have so entirely recovered the parts they had been deprived of, whether the superior or the basis, that no manner of difference could be perceived. Some of the men of learning, whom my first discoveries brought to my study, imagined that the basis was the most essential part of the animal, and that the mouth and limbs were to be considered only as extremities. I was myself inclined to adopt that notion, seeing that in all the species abovementioned, the basis ever gave the greatest marks of sensibility, that the intestines are situated in that region, &c. but who, on seeing the upper part of an anemony producing

a new basis, perfectly similar to that which had been severed from it, will any longer maintain such an opinion? During the great equinoctial tides, in places whence the sea seldom recedes, I saw several of these animals which had been cut through the middle, perhaps by some crab, or by the sudden collision of pebbles, or by some other means, which though not unnatural, we may yet not be able to account for. They soon began to recover. I should have taken them for a new species, had not my former experiments pointed out to me the gradual reproduction with which nature, no less various than impenetrable in her resources, kindly indulges them. Are not the accidents which happen to birds, quadrupeds, and even to man, frequently followed by effects, which seem intended to convince us, that we lay too great a stress on the resources of art, and trust less than we ought to do to nature? Though I could never yet arrive at any certain knowledge concerning the generation of this species, I suspect that it is different from that of the others. Several of my specimens have suddenly let fall to the bottom of the vessel, in which they were kept, a slimy substance, nearly of the colour of their bodies, perhaps somewhat yellower, which, in the microscope, appeared to consist of a great number of globular particles, pretty much resembling the spawn of fish.

The first anemonies procured of the 4th species, had probably been brought near the coast by fishermen, for they generally keep in deep water, where they are found adhering to oyster shells. I caused several to be brought into my study, where being put into sea water they soon expanded. The largest, which opened first, puzzled me not a little. I could discover no basis, but only saw limbs projecting on every side. I flattered myself that a greater expansion would clear up the difficulty; on the contrary it only added to it. The others opened, and appeared in a shape much more similar to that which I expected. I saw a basis, a body, a great number of slender limbs, the assemblage of which formed, at first, different kinds of tufts, and afterwards various fine plumes of a whitish hue inclining to carnation. I returned to my first specimen, which now appeared to consist of 2 animals joined at the basis. I became very solicitous to unravel the mystery of this singular union. At length I perceived, that this was a monster of its kind, consisting of 3 different animals blended into one. It perished 12 days after I had received it. Its internal structure, though in great confusion, was yet an interesting object to those who are acquainted with that part of this animal, and who have a taste for comparative anatomy. It appeared in such disorder, that I can scarcely conceive how it was possible for the creature to live in that condition. The state of dissolution, which began soon after, and the impossibility of representing every part at one view, prevented my taking a drawing of this remarkable inside. Its mouths on either side were regularly shaped, but rather less than the usual size; and in the folds, formed

by the bases, several limbs appeared, which seemed to belong to a 3d animal, incorporated in the 2 that were more apparent. The sequel will show, that this is not the only peculiarity observed in this species, which by its manner of propagating seems particularly calculated for producing monsters. The anemonies of this kind are commonly found adhering to the convex shells of oysters: they abound in the road of Havre-de-Grace, so that I had no difficulty in procuring whatever number I chose. A viscose matter, like that which is seen on fish newly caught, issues from them. I have opened 2 or 3 hundred of a large size, but I never found in any of them either whole or parts of animals; and yet as often as I offered them a piece of oyster or muscle, they would swallow it. The large anemonies of this species are generally surrounded by a multitude of small and middle-sized ones, which form very pleasing groups, see fig. 9, pl. 12. The bases of some of these small anemonies were not perfectly round, and in others they mutually adhered to each other: and when the basis between two connected anemonies were slightly touched with a pointed instrument, they both contracted at the same time. This common basis distended itself gradually near the middle, where it assumed the appearance of a net, which at length bursting, left every small anemony to live by itself. There issues out of the body of the anemonies of this species, through little pores, and also out of their mouths, a considerable number of round, soft, limber threads, of the thickness of a horse-hair, and of the colour of the animal. On viewing them through a lens, I observed a great resemblance between them and the spermatic vessels of men, when stripped of their outward sheath. Through a common microscope I saw fibres in them which crossed each other in every direction: and by means of a solar microscope, which magnified them to a diameter of 5 inches, they appeared of a very close texture, which, when decomposed, seemed to consist of an infinite number of vessels, crossing each other in almost every direction; but farthest extended lengthwise. A liquor seemed to circulate in the largest of these vessels, which, where they meet, form kinds of ganglions like the optic nerves in man. Such an organization cannot surely but be intended for valuable purposes. Is it not probable that these threads contain certain knots, bulbs, knobs, or buds, which open in time, and cleaving to the bodies on which those threads are extended, produce small anemonies, which at first communicate with each other, but afterwards separate by a contraction, as I have indeed observed in some of them. This I concluded from never having found any young ones in the great number of anemonies I have opened; and yet I have seen prodigious quantities, of a very small size, adhering to oyster shells. But, from a series of observations, I have learnt, among other singularities, that these animals having their bases irregularly distended, and their extremities closely adhering to some hard body, commonly an oyster shell, by suddenly shrinking, they leave



on that body some small portions of the rim of their bases, in size inferior to a lentil. These little shreds have at first no determined figure; but gradually assume the rounded shape of a drop of tallow: at length, in about 2 or 3 months, a hole appears in the middle, which forms the mouth. An internal organization, dilatations and contractions, sensibility, and other gradual improvements, soon after prove them to be animals similar to those to which they owe their origin. It might be imagined, that some time must elapse before they can grow to a circumference of 2 feet. I have not been able to follow them to that degree of increase; but I have seen them in my house, where they are far from being so favourably situated as in the sea, growing to a size large enough to convince persons of ever so little observation, that they belong to the species of large anemonies. The same shred often produces several small anemonies, which at first adhere together, and in time are separated by the little contraction already mentioned; but if they happen to remain connected, they then produce singular forms and often monsters. Besides the anemony abovementioned, the old one of this species, which has particularly unravelled this mystery, was formed in the shape of a Y, having 2 perfect bodies, of which the bases, both perforated, adhered and communicated to the same trunk; as appeared by observing that the food descended into the main trunk: neither did these 2 anemonies, thus connected, ever appear to have different inclinations, as is the case with those that are once separated. Is it not reasonable therefore to suppose, that in this state of union, every want was common, and each had its separate desire of satisfying it; and that, to keep up the habitual exercise of the animal functions in each, both were on all occasions prompted to the performance of the same function at the same time.

In order to imitate the effects of nature, I clipped several small pieces from the rim of the bases of anemonies of this species, and preserved them. Some of them became small anemonies, similar to those that had been torn off of their own accord; but many perished without producing any thing. May we not conclude from this experiment, that the prolific pieces contained a small bulb intended to become a new anemony, and to be soon after torn off by the mother; and that those which perished, either contained none of those bulbs, or such as were not sufficiently formed to thrive and grow after a violent amputation? I rather incline to this opinion, having observed, that among the pieces I had cut off, those particularly succeeded which appeared interiorly replete and of a certain thickness. If so, this conjecture may possibly lead us to another. It is well known that the fresh water polypi increase by section, and that being cut into several pieces, each of these pieces becomes an animal similar to the original one. Thus a polypus being divided in 2, 4, 8, 16, or more parts, each of these parts probably contains a bulb capable of becoming another

polypus. In the course of my experiments, the small pieces cut off from the bases and robes of the anemonies did not exceed the 500th part of the animal; it is not therefore to be wondered, if many of them did not prove prolific; they probably contained none of the fertile bulbs. The reproduction of the polypus by section will then no longer be attributed to any of its rude and shapeless parts; but rather to parts that are organized in a particular manner, to eggs, or perhaps to something more than eggs. The singular propagation of several kinds of this animal seems to favour this conjecture. In so minute an object as the fresh-water polypus, much is easily overlooked; but in the sea anemonies, though we are far from seeing every thing, yet it is possible, even without the assistance of glasses, to discern a great deal which must escape us in the most diligent examination of the other animal. The first observation of any note, was made on the 16th of June 1773. The animals of this species being very large, I only operated on young ones, and on that day cut one that was not thicker than a goose-quill. On the 30th the upper part was perfectly restored, and the fold which is seen near the upper extremity of the body of this species, appeared exactly like that I had cut off. By practice I arrived at such dexterity as to cut in two, at one snip of the shears, in a very straight line, an animal of this species as thick as my arm. This was performed on the 18th of October: before the end of the month new limbs appeared, of which the large ones, within the tufts, shot forth long before the others. On the 10th of December the animal began to eat, though its mouth was scarcely formed. The upper part was still alive. I tied a string round some of these anemonies while they were considerably extended lengthwise, and pulled the noose very tight. They had the dexterity to free themselves in a few hours of this troublesome ligament, by gradually withdrawing their upper extremity: then on measuring the noose I found it not quite 6 lines in diameter. This species is good to eat.

Among the sea anemonies brought by the fishermen, I have some reason to think that I have discovered a 5th species, which seems to reside only in places from which the sea seldom recedes. They appeared to be as small as those of the first species; their limbs, which are somewhat confusedly arranged in 3 rows, are also nearly the same. They have the form and the knobs of the 2d, and the threads of the 4th species, which latter however are coloured. Their mouths are round, and bordered with small reddish limbs; only 1 white spot is seen on one side of the mouth, whereas 2 of them appear in those of the 3d species. The middle between the mouth and the limbs is of a greenish hue, with narrow variegated streaks extending from the centre to the circumference. The specimens I have seen were white on the superior edge of the robe, of a golden yellow in the middle, inclining to a duskier colour towards the bottom; that is, the ground-colour of the robe changed gradually, from white at the top

to brown at the bottom, passing by imperceptible transitions through a succession of yellow shades, partaking more or less of the colour of gold. This whole robe was speckled with light crimson spots, and no rim appeared at the basis. These anemonies had been found on old volutes, called spindle-shells (*fucus brevis*.) Another specimen, which was found adhering to an oyster-shell, was of a darker colour; but its limbs bore some resemblance to the horns of cattle: they were of a pale green, with circles of a fine dark brown, which had a very pleasing effect. These limbs appeared, at first sight, to tend towards the centre, by the continuation of some semicircles which gradually diminished.

My very earliest observation showed that the sea anemonies feel and prognosticate, within doors, the different changes of temperature in the atmosphere. I had not leisure at that time to form tables of their various indications; but I have since done it. This fact, if applied to practice, might be of use in the formation of a sea-barometer, an object of no small importance, which several ingenious men have hitherto endeavoured in vain to furnish us with. I should prefer the anemonies of the 3d species for this purpose, their sensation being very quick; they are also easily procured, and may be kept without nourishment. Five of them may be put in a glass vessel, 4 inches wide and as many in depth, in which they will soon cleave to the angle formed by the sides and the bottom. The water must be renewed every day, and, as they do not require a great quantity of it, as much may be fetched from the sea (if they be kept on land) as will supply them for several days; its settling some time will only improve it. If the anemonies be at any time shut and contracted, I have reason to apprehend an approaching storm; that is, high winds and a rough agitated sea. When they are all shut, but not remarkably contracted, they forebode a weather somewhat less boisterous, but still attended with gales and a rough sea. If they appear in the least open, or alternately and frequently opening and closing, they indicate a mean state both of winds and waves. When they are quite open, I expect tolerable fine weather and a smooth sea. And lastly, when their bodies are considerably extended, and their limbs divergent, they surely prognosticate fixed fair weather and a very calm sea. There are times when some of the anemonies are open and others shut; the number must then be consulted; the question is decided by the majority. The anemonies used as barometers should not be fed, for then the quantity of nourishment might influence their predictions. Anemonies of this and of the first species live and do well for several years, without taking any other food but what they find disseminated in the sea water: but should a respite of some days be granted them, they might then be fed with some pieces of muscles or soft fish, and thus restored to their original vigour. Whenever the vessel is sullied by the sediments of salts, slime, the first shoots of sea-plants, &c. it may, on changing

the water, be cleansed by wiping it with a soft hair pencil, or even with the finger, carefully avoiding to rub or press hard on the anemonies. Should any of them drop off during this operation, they may be left at liberty, for they will soon, of their own accord, fix themselves to some other place. Should any of them die, which will soon be discovered by the milky colour of the water, and an offensive smell on changing it, it must be taken out, and on the first opportunity another of the same species be put in its place; those of a moderate size are the most eligible.

Explanation of Fig. 9, pl. 12, which represents a group of sea anemonies of the 4th species, adhering to an oyster-shell. N° 1, is a contracted anemony of the natural and middling size adhering to the oyster shell. N° 2, Anemonies united to the same trunk, compared in the essay to the letter  $\gamma$ . At its basis a little shred appears ready to be torn off, in order to become a new anemony. N° 3 and 4, Two young anemonies moderately expanded, in the middle of which the mouths appear. N° 5, An anemony, somewhat more grown, on which the projecting rim appears. N° 6, Eight small anemonies, 2 of which adhere together, as do also 2 others, which however are on the point of being separated by the contraction of the part that unites them. Other small anemonies, of different sizes, are seen on the oyster shell.

*XXI. On the Sea-Cow,\* and the Use made of it. By Molineux Shuldhham, Esq.*  
p. 249.

There is nothing in this paper sufficiently interesting for re-publication.

*XXII. The Process of making Ice in the East-Indies. By Sir Robert Barker,†*  
*F. R. S.* p. 252.

This paper contains an account of the method by which ice was made at Allahabad, Mootegil, and Calcutta, in the East-Indies, lying between  $25\frac{1}{4}$  and  $23\frac{1}{4}$  degrees of north latitude. At the latter place Sir R. never heard of any persons having discovered natural ice in the pools or cisterns, or in any waters collected in the roads; nor has the thermometer been remarked to descend to the freezing point; and at the former very few only have discovered ice, and that but seldom. But in the process of making ice at these places, it was usual to collect a quantity every morning, before sun-rise, except in some particular kinds of weather, for near 3 months in the year: viz. from December till February.

The ice-maker belonging to Sir R. at Allahabad, made a sufficient quantity in the winter for the supply of the table during the summer season. The methods he pursued were as follow: on a large open plain, 3 or 4 excavations were made, each about 30 feet square and 2 deep; the bottoms of which were strewed about 8 inches or a foot thick with sugar-cane, or the stems of the large Indian corn dried. On this bed were placed in rows, near each other, a number of small, shallow, earthen pans, for containing the water intended to be frozen.

\* *Trichechus Rosmarus*. Linn. † Some time commander in chief of the forces in India.

These are unglazed, scarcely a quarter of an inch thick, about an inch and a quarter in depth, and made of an earth so porous, that it was visible, from the exterior part of the pans, the water had penetrated the whole substance. Towards the dusk of the evening, they were filled with soft water, which had been boiled, and then left in the beforementioned situation. The ice makers attended the pits usually before the sun was above the horizon, and collected in baskets what was frozen, by pouring the whole contents of the pans into them, and thus retaining the ice, which was daily conveyed to the grand receptacle or place of preservation, prepared generally on some high dry situation, by sinking a pit of 14 or 15 feet deep, lined first with straw, and then with a coarse kind of blanketing, where it was beaten down with rammers, till at length its own accumulated cold again freezes and forms one solid mass. The mouth of the pit is well secured from the exterior air with straw and blankets, in the manner of the lining, and a thatched roof is thrown over the whole.

The quantity of ice depends much on the weather; so that it has sometimes happened, that no congelation took place. At others perhaps half the quantity will be frozen; and often the whole contents are formed into a perfect cake of ice: the lighter the atmosphere, and the more clear and serene the weather, the more favourable for congelation, as a frequent change of winds and clouds are certain preventives. For it is frequently remarked, that after a very sharp cold night, to the feel of the human body, scarcely any ice has been formed; when at other times the night has been calm and serene, and sensibly warmer, yet the contents of the pans will be frozen through. The strongest proof of the influence of the weather appears by the water in one pit being more congealed than the same preparation for freezing will be in other situations, a mile or more distant.

The climate may probably contribute in some measure to facilitate the congelation of water, when placed in a situation free from the heat of the earth, since those nights in which the greatest quantity of ice has been produced, were, as before observed, perfectly serene, the atmosphere sharp and thin, with very little dew after midnight. The spongy nature of the sugar-canes, or stems of the Indian corn, appears well calculated to give a passage under the pans to the cold air; which, acting on the exterior parts of the vessels, may carry off by evaporation a part of the heat. The porous substance of the vessels seems equally well qualified for the admission of the cold air internally; and their situation being full a foot beneath the plane of the ground, prevents the surface of the water from being ruffled by any small current of air, and thus preserves the congealed particles from disunion. Boiling the water is esteemed a necessary preparative to this method of congelation; but how far this may be consonant with philosophical reasoning, Sir R. presumes not to determine.

From these circumstances it appears, that water, by being placed in a new situation free from receiving heat from other bodies, and exposed in large surfaces to the air, may be brought to freeze when the temperature of the atmosphere is some degrees above the freezing point on the scale of Fahrenheit's thermometer; and by being collected and amassed in a large body, is thus preserved, and rendered fit for freezing other fluids, during the severe heats of the summer season. In effecting which, there is also an established mode of proceeding; the sherbets, creams, or whatever other fluids are intended to be frozen, are confined in thin silver cups of a conical form, containing about a pint, with their covers well luted on with paste, and placed in a large vessel, filled with ice, saltpetre, and common salt, of the two latter an equal quantity, and a little water to dissolve the ice and combine the whole. This composition presently freezes the contents of the cups to the same consistency of our ice creams, &c. in Europe; but plain water will become so hard as to require a mallet and knife to break it. On applying the bulb of a thermometer to one of these pieces of ice, thus frozen, the quicksilver has been known to sink 2 or 3 degrees below the freezing point; so that from an atmosphere apparently not cold enough to produce natural ice, ice shall be formed, collected, and a cold accumulated, that shall cause the quicksilver to fall even below the freezing point.

*XXIII. Of the House-Swallow, Swift, and Sand-Martin. By the Rev. Gilbert White. p. 258.*

This excellent paper is reprinted in Mr. White's History of Selborne, to which the reader is referred.

*XXIV. Of a Machine for raising Water, executed at Oulton, in Cheshire, in 1772. By Mr. John Whitehurst. p. 277.*

Presuming the mode of raising water by its momentum may be new and useful to many individuals, Mr. W. was induced to send a description of a work, executed in 1772, at Oulton, Cheshire, the seat of Philip Egerton, Esq. for the service of a brewhouse and other offices, which was found to answer effectually. The circumstances attending this water-work require a particular attention, and are as follow. *A*, in fig. 10, pl. 12, represents the spring or original reservoir, its upper surface coinciding with the horizontal line *bc*, and with the bottom of the reservoir *k*. *d* the main pipe, 1½ inch diameter, and nearly 200 yards in length. *e* a branch pipe, of the same diameter, for the service of the kitchen offices, situated at least 18 or 20 feet below the surface of the reservoir *A*; and the cock *F* was about 16 feet below it. *g* represents a valve-box, *g* the valve, *h* an air vessel, *oo* the ends of the main pipe inserted into *h*, and bending downwards, to prevent the air from being driven out when the water is forced into it

w the surface of the water. Now it is well known, that water discharged from an aperture, under a pressure of 16 feet perpendicular height, moves at the rate of 32 feet in a second of time; therefore such will be the velocity of the water from the cock *F*. And though the aperture of the cock *F* is not equal to the diameter of the pipe *D*, yet the velocity of the water contained in it will be very considerable: consequently, when a column of water, 200 yards in length, is thus put into motion, and suddenly stopped by the cock *F*, its momentous force will open the valve *g*, and condense the air in *H*, as often as water is drawn from *F*. In what degree the air is thus condensed, is needless to say in the instance before us; therefore Mr. W. only observes, that it was sufficiently condensed to force out the water into the reservoir *K*, and even to burst the vessel *H*, in a few months after it was first constructed, though apparently very firm, being made of sheet lead, about 9 or 10 pounds weight to a square foot. Whence it seems reasonable to infer, that the momentous force is much superior to the simple pressure of the column *IK*; and therefore equal to a greater resistance, if required, than a pressure of 4 or 5 feet perpendicular height. It seems necessary further to observe, that the consumption of water in the kitchen offices is very considerable; that is, that water is frequently drawing from morning till night all the days of the year.

*XXV. On Occultations of Stars and Geometrical Theorems. Being an Extract of a Letter from Mr. Lexel, to Dr. Morton. Dated Petersburg, June 14, 1774. p. 280.*

As I propose, says Mr. L., to make some researches concerning the difference of the meridians of the principal Observatories of Europe, which I am persuaded can best be ascertained by the occultations of the fixed stars by the moon; it would be of great service to me to be furnished with the observations that have been made, or that will be made, this year, of the occultations of  $\alpha$  or of  $\gamma$  Tauri by the moon. I beg therefore Sir, you will please to desire Mr. Maskelyne to communicate them to me, towards the beginning of the next year, directed to Mr. Euler, secretary of our Academy. It would also be of great use to me to have the observation of the occultation of the Pleiades by the moon the 15th of March, 1766, in case it has been taken at Greenwich. The following are some observations of Mr. Wargentin, of the occultations of  $\alpha$  and  $\gamma$  Tauri.

1773, Nov. 1....	11 <sup>h</sup> 56 <sup>m</sup> 12 <sup>s</sup> ....	Emersion of $\alpha$ , uncertain to some seconds.
1774, Jan. 22....	6 0 26 $\frac{1}{2}$ ....	Immersion of the eye of $\delta$ , } both very certain,
....	7 15 51....	Emersion,
Feb. 18....	6 39 51....	Immersion of $\gamma$ , very certain.
	7 19 33....	Emersion, within 2 seconds.

The following are my observations.

1773, Nov.	1....	12 <sup>a</sup>	56 <sup>m</sup>	47 <sup>s</sup>	....	{ Emersion of $\alpha$ almost certain; the immersion was not observed on account of clouds.
1774, Jan.	22....	7	2	52.....	Immersion,	} both certain.
		8	20	44.....	Emersion,	
April	14....	8	28	34.....	Immersion of $\alpha$ , very certain.	
		9	3	20.....	Emersion of the same.	
	15....	9	32	0.....	Immersion of Flamstead's 115 in $\gamma$ .	
	16....	10	21	31.....	Immersion of a star of the 6th magnitude in $\Pi$ .	
May	22....	13	2	20.....	Immersion of $m$ Virginis, very certain.	

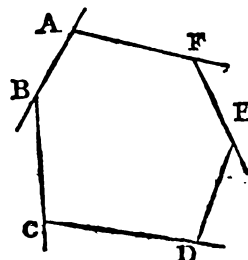
I have lately discovered two curious theorems, which I shall here communicate to the R. S.

*Theorem.*—Let A, B, C, D, E, F, be a polygon whose sides are named  $a, b, c, d, e, f$ ; and the exterior angles  $\alpha, \beta, \gamma, \delta, \epsilon, \zeta$ , so that the side  $a$  be placed between the angles  $\alpha$  and  $\beta$ ,  $b$  between  $\beta, \gamma$ , &c.

$$1. a \times \sin. \alpha + b \times \sin. (\alpha + \beta) + c \times \sin. (\alpha + \beta + \gamma) + d \times \sin. (\alpha + \beta + \gamma + \delta) + e \times \sin. (\alpha + \beta + \gamma + \delta + \epsilon) + f \times \sin. (\alpha + \beta + \gamma + \delta + \epsilon + \zeta) = 0.$$

$$2. a \times \cos. \alpha + b \times \cos. (\alpha + \beta) + c \times \cos. (\alpha + \beta + \gamma) + d \times \cos. (\alpha + \beta + \gamma + \delta) + e \times \cos. (\alpha + \beta + \gamma + \delta + \epsilon) + f \times \cos. (\alpha + \beta + \gamma + \delta + \epsilon + \zeta) = 0.$$

In fact it is  $\sin. (\alpha + \beta + \gamma + \delta + \epsilon + \zeta) = \sin. 360^\circ = 0$ , and  $\cos. (\alpha + \beta + \gamma + \delta + \epsilon + \zeta) = +1$ ; but in order to give the same form to the two expressions, I rather chose to represent them as I have done. By means of these two theorems the solution of polygons will be as easy as that of triangles by common trigonometry.



*XXVI. Investigation of a General Theorem for finding the Length of any Arc of any Conic Hyperbola, by Means of two Elliptic Arcs. With some other New and Useful Theorems deduced from it. By J. Landen, F.R.S. p. 288.*

1. From the theorem noticed in art. 1 of the author's paper in the Philos. Trans., 1771, (p. 150, of this abridged vol.) it follows, that in the hyperbola AD (pl. 12, fig. 11), if the semi-transverse axis AC be  $= m - n$ ; the semi-conjugate  $= 2\sqrt{mn}$ ; and the perpendicular CP, from the centre c on the tangent DP,  $= \sqrt{(m - n)^2 - t^2}$ ; the difference DP - AD, between the said tangent DP and the arc AD, will be equal to the fluent of  $\sqrt{\frac{(m - n)^2 - t^2}{(m + n)^2 - t^2}} \times t$ .

2. It is well known, that in any ellipsis whose semi-transverse axis is  $m$ , and semi-conjugate  $n$ ; if  $x$  be the abscissa, measured from the centre on the transverse axis, and  $z$  the arc between the conjugate axis and the ordinate corresponding to  $x$ ,  $\sqrt{\frac{m^2 - x^2}{m^2 - n^2}} \times x$  will be  $= z$ ,  $g$  being  $= \frac{m^2 - n^2}{m^2}$ .

Hence,  $\sqrt{\frac{(m + n)^2 - t^2}{(m - n)^2 - t^2}} \times t$  being  $= \sqrt{\frac{(m + n)^2 - t^2}{(m + n)^2 - (\frac{m + n}{m - n})^2 t^2}} \times \frac{m + n}{m - n} t$ , it ap-



pears, that in the ellipsis aed (fig. 12) whose semi-transverse axis cd is  $= m + n$ , semi-conjugate ca  $= 2\sqrt{mn}$ , and abscissa cb (corresponding to the ordinate be)  $= \frac{m+n}{m-n}t$ ; the arc ae is equal to the fluent of  $\sqrt{\frac{(m+n)^2 - t^2}{(m-n)^2 - t^2}} \times t$ .

3. In the ellipsis aefd (fig. 13), the semi-transverse axis cd being  $= m$ ; the semi-conjugate ca  $= n$ ; and the abscissa cb (corresponding to the ordinate be)  $= x$ ; if ep, the tangent at e, intercepted by a perpendicular (cp) drawn to it from the centre c, be denoted by  $t$ ;  $gx \times \sqrt{\frac{m^2 - x^2}{m^2 - g^2}}$  (as is well known) will be  $= t$ ,  $g$  being as in the preceding article.

Hence  $x^2 = \frac{m^2g + t^2}{2g} - \sqrt{\frac{(m^2 - n^2)^2 - 2 \times (m^2 + n^2) \times t^2 + t^4}{2g}}$ . From which equation, by taking the fluxions, we have,  $x\dot{x}$

$$= \frac{\dot{t}}{2g} + \frac{(m^2 + n^2) \times \dot{t} - t^2 \dot{t}}{\sqrt{[2g(m^2 - n^2)^2 - 2 \times (m^2 + n^2) \times t^2 + t^4]}} = \frac{\dot{t}}{2g} + \frac{(m^2 + n^2) \times \dot{t} - t^2 \dot{t}}{\sqrt{[2g(m-n)^2 - t^2 \times (m+n)^2 - t^4]}}$$

But  $\dot{z}$  being  $= \sqrt{\frac{m^2 - g^2}{m^2 - x^2}} \times \dot{x}$ , as observed in the preceding article, it appears

$$\text{that } \frac{g}{t} \times x\dot{x} \text{ is } = \dot{z}. \text{ It is obvious therefore that } \dot{z} \text{ is } = \frac{1}{4}\dot{t} + \frac{1}{4} \times \frac{(m^2 + n^2) \times \dot{t} - t^2 \dot{t}}{\sqrt{[(m-n)^2 - t^2] \times [(m+n)^2 - t^2]}} \\ + \frac{1}{4} \times \frac{(m-n)^2 \times \dot{t} - t^2 \dot{t}}{\sqrt{[(m-n)^2 - t^2] \times [(m+n)^2 - t^2]}} = \frac{1}{4}\dot{t} + \frac{1}{4} \sqrt{\frac{(m-n)^2 - t^2}{(m+n)^2 - t^2}} \times \dot{t} + \frac{1}{4} \sqrt{\frac{(m+n)^2 - t^2}{(m-n)^2 - t^2}} \times \dot{t}. \text{ Whence, taking the fluents by the theorems in art. 1 and 2, we have } z = ae \text{ (fig. 13)} = \frac{1}{4}t + \frac{DP - AD}{4} \text{ (fig. 11)} + \frac{ae}{4} \text{ (fig. 12); consequently the hyperbolic arc AD is } = DP + ae + 2t - 4ae. \text{ Thus, beyond my expectation, I find, that the hyperbola may in general be rectified by means of two ellipses.}$$

Writing  $E$  and  $F$  for the quadrantal arcs ad, ad (fig. 12 and 13) respectively, and  $L$  for the limit of the difference  $DP - AD$ , while the point of contact ( $D$ ) is supposed to be carried to an infinite distance from the vertex  $A$  of the hyperbola (fig. 11), we find  $2F - E = L$ , the value of  $ae$  being  $= \frac{1}{4}F + \frac{1}{4}m - \frac{1}{4}n$  when  $t$  is  $= m - n$ ; that is, when  $e$  coincides with  $d$  (fig. 12), and  $F$  with  $c$  (fig. 11), by what I have proved in the before-mentioned paper, art. 10.

4. From what is done above, the following useful theorems are deduced.

Theorem 1. The fluent of  $\frac{1}{4}a^{\frac{1}{2}}z^{-\frac{1}{2}}\dot{z} \sqrt{\frac{\frac{b^2}{a^2} + z}{a - z}}$  is  $= de$ .

Theorem 2. The fluent of  $\frac{1}{4}a^{\frac{1}{2}}z^{-\frac{1}{2}}\dot{z} \times \sqrt{\frac{a - z}{\frac{b^2}{a^2} + z}} = (\frac{2a^2}{b^2} + 1)de - (\frac{2a^2}{b^2} + 2)ef$ .

Theorem 3. The fluent of  $\frac{\frac{1}{4}a^{\frac{1}{2}}z^{\frac{1}{2}}\dot{z}}{\sqrt{(b^2 + 2kz - z^2)}} = 2ef - de = 2F - E + AD - DP$ .

Theorem 4. The fluent of  $\frac{\frac{1}{4}a^{-\frac{1}{2}}b^{\frac{1}{2}}z^{-\frac{1}{2}}\dot{z}}{\sqrt{(b^2 + 2kz - z^2)}} = 2(de - ef)$  N.B.  $k = \frac{a^2 - b^2}{2a}$ .

These theorems still refer to fig. 11, 12, 13; but now the values of the several lines in them (being not as before) are as here specified, viz. Fig. 11, in the hyperbola AD, the semi-transverse axis AC is now =  $a$ ; the semi-conjugate =  $b$ ; the perpendicular CP, from the centre c on the tangent DP, is =  $\sqrt{az}$ , the said tangent DP =  $\sqrt{\frac{a}{z}} \times (b^2 + 2hz - z^2)$ ; and the abscissa CB (corresponding to the ordinate BD) is =  $a\sqrt{\frac{a}{z}} \times \sqrt{\frac{az + b^2}{a^2 + b^2}}$ .

Fig. 2. In the ellipses aed, the semi-transverse axis cd is =  $\sqrt{a^2 + b^2}$ ; the semi-conjugate ca =  $b$ ; the abscissa cb =  $\sqrt{\frac{a^2 + b^2}{a}} \times \sqrt{(a - z)}$ ; and the ordinate be =  $b\sqrt{\frac{z}{a}}$ .

Fig. 13. In the ellipsis aefd, the semi-transverse axis cd is =  $\frac{1}{2}\sqrt{(a^2 \times b^2) + \frac{1}{4}a^2}$ ; the semi-conjugate ca =  $\frac{1}{2}\sqrt{(a^2 + b^2)} - \frac{1}{4}a$ ; the tangents ep, fq, intercepted by perpendiculars (cp, cq) drawn to them from the centre c, each =  $\sqrt{a} \times (a - z)$ ; and the abscissa (cb' or cb'') on cd, corresponding to the point e or f, 
$$\frac{\sqrt{[\sqrt{(a^2 + b^2)} + a - z \mp \sqrt{(z^2 + \frac{b^2}{a}z)]}}{\sqrt{2 + \sqrt{(a^2 - b^2)}}} \times cd.$$

The quadrantal arc ad (fig. 12) is denoted by E; and the quadrantal arc ad (fig. 13) is denoted by F; L the limit of DP - AD (fig. 11) is =  $2F - E$ .

From what is now done, I might proceed to deduce many other new theorems, for the computation of fluents; but I shall at present decline that business: and, after giving a remarkable example of the use of theorem 4, in computing the descent of a heavy body in a circular arc, conclude this paper with a few observations relative to the contents of the preceding articles.

5. Let lpqn (fig. 14) be a semi-circle perpendicular to the horizon, whose highest point is l, lowest n, and centre m. Let ps, qt, parallel to the horizon, meet the diameter lmn in s and t: and let the radius lm (or mn) be denoted by  $r$ , the height ns by  $d$ ; and the distance st by  $x$ . Then, putting  $h$  for  $(16\frac{1}{4}$  feet) the space a heavy body, descending freely from rest, falls through in one second of time; and supposing a pendulum, or other heavy body, descending by its gravity from p, along the arc pqn, to have arrived at q; the fluxion of the time of descent will be =  $\frac{\frac{1}{2}rh - \frac{1}{2}x - \frac{1}{2}z}{\sqrt{[2rd - d^2 - 2(r - d)(d - x)]}}$ . The fluent of which, or the time of descent from p to q is, by theorem 4 of the preceding article, =  $\frac{2r}{\sqrt{h} \times (2r - d)} \times de - ef$ ;  $a$  (in that theorem) being taken =  $\sqrt{d}$ ,  $b = \sqrt{(2r - a)}$ ,  $cb$  (fig. 2) =  $\sqrt{\frac{2r}{d}} \times \sqrt{(d - x)}$ , and ep, fq, (fig. 13) each =  $\sqrt{(d - x)}$ . Hence it appears, that the whole time of descent from p to n is =  $\frac{2r}{\sqrt{h} \times (2r - d)} \times (E - F)$ ; when, in fig. 12 and 13, the semi-axes are taken according to the values of  $a$  and  $b$  just now specified.

6. If  $pqn$  be a quadrant; that is, if  $d\text{ be } = r$ , the whole time of descent from  $p$  to  $n$  will be  $= \frac{2}{\sqrt{h}} \times (E - F)$ , by the above theorem. Which time, by what I have shown in the Philos. Trans. for 1771, is  $= \frac{1}{\sqrt{h}} \times (\frac{1}{2}E + \frac{1}{2}\sqrt{E^2 - 2c})$ ,  $c$  being  $\frac{1}{2}$  of the periphery of the circle whose radius is  $r$ . Consequently,  $\frac{2}{\sqrt{h}} \times (E - F)$  being found  $= \frac{1}{\sqrt{h}} \times (\frac{1}{2}E + \frac{1}{2}\sqrt{E^2 - 2c})$ , we find from that equation  $F = \frac{1}{2}E - \frac{1}{2}\sqrt{E^2 - 2c}$ , where  $E$  is the quadrantal arc of the ellipsis, whose semi-transverse and semi-conjugate axes are  $\sqrt{2r}$  and  $\sqrt{r}$ ; and  $F$  the quadrantal arc of another ellipsis, whose semi-transverse and semi-conjugate axes are  $\sqrt{\frac{r}{2}} + \frac{1}{2}\sqrt{r}$  and  $\sqrt{\frac{r}{2}} - \frac{1}{2}\sqrt{r}$ .

Before Mr. Maclaurin published his Treatise of Fluxions, some eminent mathematicians imagined that the elastic curve could not be constructed by the quadrature or rectification of the conic sections. But that gentleman has showed in that treatise, that the said curve may in every case be constructed by the rectification of the hyperbola and ellipsis; and he has observed that, by the same means, we may construct the curve along which, if a heavy body moved, it would recede equally in equal times from a given point. Which last mentioned curve Mr. James Bernoulli constructed by the rectification of the elastic curve, and Mr. Leibnitz and Mr. John Bernoulli by the rectification of a geometrical curve of a higher kind than the conic sections. It is observable, that Mr. Maclaurin's method of construction just now adverted to, though very elegant, is not without a defect. The difference between the hyperbolic arc and its tangent being necessary to be taken, the method always fails when some principal point in the figure is to be determined; the said arc and its tangent then both becoming infinite, though their difference be at the same time finite. The contents of this paper properly applied, will evince that both the elastic curve, and the curve of equable recess from a given point, with many others, may be constructed by the rectification of the ellipsis only, without failure in any point.

*XXVII. Astronomical Observations made at Chislehurst, in Kent, in the Year 1774. By the Rev. Francis Wollaston, LL.B., F. R. S. p. 290.*

Mr. W. having now completed his original design, and kept his clock going for a 3d year, without the least touch of the oil, or any alteration whatever, he presumes the result of his observations to ascertain the rate of its going, may not be an unacceptable addition to the former papers on that subject, delivered to the Society. The regular difference between the summer and winter months, and some degree of similarity between those differences, seems to show a regularity in the cause. What that may be, is not fully to be ascertained by these observations; though it seems to have been difference of moisture, rather than

of heat. By comparing these last 3 years with that first given, when the clock was in some degree foul, it seems as if it were most affected when the work is clean. Though that is not quite certain; for the differences, which decreasing gradually in the following table, would justify this conclusion, it may be observed increase again in the last instance. For hence it appears that July and August are the months for greatest acceleration, and Jan. and Feb. for retardation; contrary to the affection of metalline rods, but agreeable to the effect to be expected from moisture on wood. Yet this difference is not so great in any degree, nor (what is more material to observation) by any means so sudden in its changes, as what is occasioned by heat on metals. And even this perhaps might be obviated by a strong coat of varnish on the rod, or some preparation of the wood itself. One thing it may be proper to mention, as an accidental experience Mr. W. had the last year; that a clock so fixed, with a pendulum of so simple construction, is not easily affected by any tremulous motion of the building to which it is fastened. In the months of March, April, and part of May, he had occasion to make alterations in the top of his house, in order to gain more rooms in it; and notwithstanding the great jarring necessarily consequent on taking off the old rafters, and laying on a new leaded roof, and new joists and floor over the observatory itself, the clock seemed not to have been disordered at all by it.

*XXVIII. Of Triangles described in Circles and about them. By John Stedman, M.D. p. 296.*

PROP. 1. *An equilateral triangle inscribed within a circle is larger than any other triangle that can be inscribed within the same circle.*—Let  $ABC$ , fig. 15, pl. 12, be an equilateral triangle, inscribed in the circle  $ADCB$ ; and let  $ADE$  be a triangle supposed larger than  $ABC$ . Let  $ADE$  be drawn with one of its angles at the same point with one of the angles of the equilateral triangle, suppose at  $A$ , and then its other two angles will fall either on the segments  $ADB$  and  $AEC$ , or one of the angles on the segment  $BC$ . First, let one of its angles fall at  $D$ , between  $A$  and  $B$ ; and the other at  $E$ , between  $A$  and  $C$ ; and draw the line  $BE$ . In the triangles  $ABC$ ,  $ABE$ , the triangle  $ABF$  is common, and the two remaining triangles  $BFC$ ,  $AFE$ , are similar; for the angle  $AFE$  is equal to its opposite angle  $BFC$ ; and the two angles  $EAC$ ,  $EBC$ , are equal, being subtended by the same segment  $EC$ , and so the two remaining angles  $AEF$ ,  $BCF$ , must be equal; therefore the sides are proportional, and  $BC$  and  $AE$ , subtending equal angles, must be homologous; but  $BC$  is equal to  $AC$ , which is greater than  $AE$ ; consequently the triangle  $BFC$  is greater than  $AFE$ , and so the equilateral triangle  $ABC$  is greater than the triangle  $ABE$ . In the same manner, the triangle  $ABE$  may be proved greater than  $ADE$ ; for  $AHE$  is common, and the two triangles  $ADH$ ,  $BHE$  are similar, and their sides proportional; and  $AD$  and  $BE$ , subtending equal

angles, must be homologous; but  $BE$  is greater than  $BC$ , which is equal to  $AB$ , and that again greater than  $AD$ ; consequently  $BE$  is greater than  $AD$ , and the whole triangle  $AEB$  greater than  $AED$ ; and so the equilateral triangle must, *a fortiori*, be greater than  $AED$ . Q.E.D.

Next, let the triangle  $ADE$  be supposed greater than the equilateral triangle  $ABC$ ; and let the angle  $ADE$  fall somewhere in the segment  $BDC$ , (fig. 16,) so that the segment  $EC$  may be greater than  $BD$ ; for if it were not, the angle  $AED$  being applied to any of the angles of the equilateral triangle, the demonstration would become the same as in the first case: therefore the segments  $AEC$ ,  $BDC$ , being equal, and  $BD$  being less than  $EC$ ,  $AE$  must be less than  $DC$ . Draw the right line  $DC$ ; then, in the two triangles  $ADC$ ,  $ADE$ , the triangle  $AFD$  is common, and the two triangles  $AFE$ ,  $DFC$  are equiangular and similar, and the sides  $AE$ ,  $DC$ , subtending equal angles, are homologous; but  $DC$  is greater than  $AE$ ; so the triangle  $DFC$  is greater than the triangle  $AFE$ , and the whole triangle  $ADC$  is greater than  $ADE$ ; but the equilateral triangle may be proved greater than  $ADC$  from the first case, and consequently greater than  $ADE$ . Q.E.D.

PROP. 2. *An equilateral triangle described about a circle is less than any other triangle that can be described about the same circle.*—Let the equilateral triangle  $ABC$ , fig. 17, be described about the circle  $HIK$ , and let the triangle  $BDG$  be supposed less than the equilateral triangle. Draw the line  $AF$  parallel to  $BC$ ; then the triangles  $AFE$ ,  $EGC$ , are similar; for the opposite angles  $ABF$ ,  $GEC$ , are equal, as likewise the angle  $AFE$  to the angle  $EGC$ ; the lines  $AF$  and  $GC$  being parallel, and falling on the same line  $FG$ , the angles  $AFE$  and  $EGC$  are therefore equal, and the sides  $AE$ ,  $EC$ , subtending equal angles, are homologous; but the side of the equilateral triangle  $AC$  being equally divided at  $I$ , the line  $AE$  is greater than  $EC$ , and consequently the triangle  $AFE$  is larger than the triangle  $EGC$ ; and the triangle  $DAE$  much larger than  $EGC$ : therefore, in the triangles  $DBG$  and  $ABC$ , the part  $ABGE$  being common, the whole triangle  $DBG$  is larger than the equilateral triangle. Q.E.D.

Whatever other triangles can be described about a circle, may be demonstrated to be larger than an equilateral triangle described about the same circle, on the same principles as the preceding.

PROP. 3. *The square of the side of an equilateral triangle, inscribed in a circle, is equal to a rectangle under the diameter of the circle, and a perpendicular let fall from any angle of the triangle on the opposite side.*—The two triangles  $ADC$ ,  $AEC$ , fig. 18, are equiangular and similar, the angles  $ACD$ ,  $AEC$ , being both right, and that at  $A$  common; therefore  $AD : AC :: AC : AE$ , and  $AC^2 = AD \times AE$ . Q.E.D.

The square of one side of the triangle being compleated, so as to include the triangle; then that part of the side of the square that falls within the circle is

equal to the radius; and the other part, lying without the circle, is equal to the radius minus twice the portion lying between the side of the square, and the circumference of the circle; or is equal to that part of the radius that lies between the centre and the side of the square minus the remainder of the radius; that is  $CL$  is equal to the radius, and  $LI = KG - 2MG$ ; or  $LI = KM - MG$ .  $FG$  being parallel to  $BC$ , and consequently perpendicular to  $IC$ , must divide the chord  $LC$  in two equal parts; so that  $MC$  being equal to  $KE$ ,  $LC$  must be equal to  $2KE$ ; but  $KE$  (by Eucl. I. xiii, pr. 12, cor. 2. Clav.) is equal to  $ED$ ; therefore  $LC = KD$  the radius. The side of the square  $IC$ , being equal to  $BC$ , is likewise equal to  $NM$ ; but  $LC$  being equal to  $KG$ , the remaining part  $LI$  must be equal to  $NK - MG$ ; or to  $KM - MG$ . Q.E.D.

*XXIX. On Polygons of the Greatest and Least Area, or Perimeter, inscribed in a Circle, or circumscribed about a Circle. By S. Horsley, LL.D., Sec. R. S. p. 301. Translated from the Latin.*

*Theorem 1.* If a right line touch a circular arc intercepted by two tangents; then its segments intercepted by its point of contact and those tangents, will be equal or unequal, according as the arc is equally or unequally divided by the point of contact. And the greater or less segments of the arc (when unequally divided) and of the right line, lie on the same side of the dividing point.—Thus, if the right line  $BD$ , fig. 1 and 2, pl. 13, in the point  $E$  touch the circular arc  $AEC$ , intercepted by the two tangents  $AB$ ,  $CD$ . Then the right line  $BD$  will be equally or unequally divided in the point  $E$ , accordingly as the arc  $AEC$  is equally or unequally divided at the same point  $E$ . So that, when  $AE = CE$ , then  $BE = DE$ , as in fig. 1; but when  $AE$  is greater than  $CE$ , then  $BE$  is greater than  $DE$ , as in fig. 2.

*Theorem 2.* Of all the right lines which touch a circular arc, and meet two other tangents at the extremities of the arc; that is the least which touches the arc in its middle point.—Thus, of all the lines,  $AC$ ,  $GH$ , touching the arc  $BED$ , fig. 3, and intercepted by the two tangents  $BAG$ ,  $DHC$ , that  $AC$  is the least which touches the arc in its middle point  $E$ .

*Theorem 3.* Of all the polygons, of a given number of sides, and circumscribing a given circle, the equiangular one has the least perimeter.

*Theorem 4.* Of all the polygons, having a given number of sides, and circumscribing a given circle, the equiangular one has the least area.

*Theorem 5 and 6.* Of all polygons, having a given number of sides, and inscribed in a given circle, the equilateral one has the greatest perimeter and area. All which theorems Dr. Horsley demonstrates with his usual geometrical rigour.

XXX. *Of an extraordinary Acephalous Birth.* By W. Cooper. M.D. p. 311.

Mrs. Brackett, of Clerkenwell-close, aged 23 years, was, at the end of her first pregnancy, by a natural labour, delivered of a perfect female child, on Friday the 8th of October, 1773, at 7 o'clock in the morning. The attending midwife, Mrs. Ayres, soon perceived by the abdominal tumour that there was another child. After waiting about 3 hours, a flooding came on; but without pain, or any advancement of the 2d delivery. The hæmorrhage producing faintness, debility, and danger, the attendants and midwife were alarmed, and Dr. C. was sent for. When he came, he found her in the situation above described; and therefore thought it his duty to accomplish the remaining part of the labour, as soon as he could, consistently with the safety of the mother. On all occasions, when the concomitant circumstances render it necessary to turn a child in utero, it is of the utmost consequence to understand, as nearly as we can, its general situation, in order to deliver with the greater ease, safety, and expedition. And to an experienced accoucheur, if the breech, knees, or feet, do not immediately present themselves, the head and face of the child will, in most cases, be a sufficient index to the position of the other parts of its body. This circumstance arises from the fœtus commonly coiling itself up into an oblong, oval, snug, compact figure, with its knees towards its chin, in order to take up as little room as possible, by being adapted to the cavity of the uterus. In the present case, when the patient was placed in a proper situation, having introduced his hand as gently as possible through the vagina, cervix uteri, and enveloping membranes, and no part of the inferior extremities or breech presenting itself, Dr. C. examined carefully for the head of the child, as usual, but without success. This disappointment somewhat embarrassed him. But as the woman's situation was become very serious by the increasing uterine hæmorrhage, he attempted without delay to get at the feet. He easily secured one of them; but though he made use of very little force in bringing it towards the os externum, the structure was so very tender that the tibia began to give way at its superior epiphysis. On this account he was reduced to the disagreeable necessity of again introducing his hand into the uterus; and as one leg had thus unexpectedly failed him, he thought it extremely futile to attempt any thing with the other. The most eligible resource which he apprehended he had now left, was to fix a blunt hook on one groin, and, when it was brought low enough, to assist gently at the other, with the 2 fore-fingers of his right hand. By these means he happily accomplished the delivery of the remaining fœtus, which proved to be a very singular kind of monster. And as the late ingenious Mr. Hewson injected its blood vessels, and dissected it, Dr. C. was enabled to attempt a short anatomical description of it, for the satisfaction of the curious in philosophy and physiology.

This extraordinary animal production was of the size and appearance of a common twin child at its full time, excepting the particularities now to be pointed out. When first born it was very plump, but soft and flabby, and the bones remarkably small and tender. It had neither head, neck, hands, nor arms. In the place where the neck should originate, was a little mammilla, somewhat larger than a woman's nipple, but quite soft. And on each side, in the place where the arm should begin, there was a small papilla, about the size, and very much like the extremity of a common quill. The spine seemed perfect, but ended abruptly at the upper vertebræ colli. Below the navel the parts were nearly entire, except the feet, where the toes were of an irregular form and size, and some of them united together. The external parts of generation, which indicated it to be a female, were also perfect. On a careful inspection internally, there was evidently no brain nor spinal marrow. A few nerves however were scattered about the abdomen; but their origin, through fear of destroying the preparation, was not traced. The uterus was perfect; but only one ovarium could be found. There was also the appearance of a bladder; but it was so contracted as to have no cavity. A large intestine arose from the anus; was a good deal convoluted above the brim of the pelvis, and ended in a blind pouch or cul de sac, on the left side of the abdomen. This viscus appeared to be about 6 or 7 inches in length, varied its size in different parts, gradually became smaller towards its superior extremity, and seemed fully distended with a colourless mucus.\* All above the navel was extremely defective. There was no heart, lungs, diaphragm, stomach, liver, kidneys, spleen, pancreas, nor small intestines. However, there were 3 small glands in the place of the thymus, whose substance, when examined with a microscope, Mr. Hewson remarked, exactly resembled that of the thymus itself. And on each side of the vena cava, just under the navel, were 2 little glandular substances, which seemed to be somewhat like capsulæ renales, only very small to what are commonly found.† There was a large artery running on the spine, which might be called the aorta. As this approached the upper extremity of the little animal, it was divided into smaller and smaller branches; and in its course it distributed lateral ones also to the contiguous parts of the trunk. Below the navel it sent off 2 branches that

\* Does not this circumstance almost amount to a proof, that the meconium, universally found in the bowels of new-born children, is nothing more than the mucus naturally secreted by the intestinal glands, mixed with bile, and perhaps a small portion of the pancreatic juice? In the present instance, as there was no liver there could be no bile, and consequently the meconium, if Dr. C. might so call it, was colourless.—Orig.

† Mr. Hewson, some time before his death, seemed to be confirmed in the opinion, that whenever children are born with little or no brain, the capsulæ renales are always very much diminished. This is certainly the case in 1 or 2 almost brainless children which Dr. C. had by him, and whose renal capsulæ he examined, with a view of being further satisfied on this subject.—Orig.



constituted the umbilical arteries, one of which was considerably larger than the other. And then below these, 2 other branches descend to the inferior extremities. A large umbilical vein came in at the navel, and was immediately divided into 2 considerable branches; one ascending, the other descending. Each of these was again subdivided into smaller and smaller branches, which, as they passed upwards and downwards, seemed to correspond with the different ramifications of the ascending and descending aorta. The funis umbilicalis was only about 2 inches in length,\* and so very tender also, that it unavoidably separated near the navel of the child during the delivery. Whether therefore there was any pulsation in this short funis, he was not able to determine. The placenta was not particularly examined.

There were evidently in this foetus 2 distinct systems of vessels, arteries and veins,† that carried red blood.‡ It was plain also, that the blood passed from the internal iliac arteries, through the hypogastrics and umbilicals to the placenta, and was returned from it by the umbilical vein to the navel, and thence distributed in the manner before observed. But as there was no heart, nor any thing analogous to one, it became extremely difficult to ascertain the powers by which the circulation was carried on through this physiological phenomenon, Might we not however venture to advance a conjecture, that the peristaltic, or living muscular power of the arteries, was principally subservient to this important end? Many examples are to be met with in the collections of the curious and learned in the different parts of Europe,§ which are somewhat similar to that now related. When carefully examined however, excepting a very few instances, they are generally found either essentially to differ, or else their structure has not been, with any tolerable precision, explained. The present history

\* An exactly similar circumstance to this Dr. C. took particular notice of, in the delivery of another almost brainless monster.—Orig.

† Mr. Hewson attempted to inject the whole blood vessels by the umbilical vein as usual. To his great surprize, no part of the injection returned by the umbilical arteries. He could not account for this singularity at that time: but as only a part of the vessels were filled, he injected afresh by one of the hypogastric arteries. On dissection afterwards, this mystery was unravelled by the heart's being totally absent. It then appeared also, that by the first injection he had filled the venal system, and by the latter the arterial.—Orig.

‡ See a very curious case related by Mons. Winslow, in the *Memoires de L'Academie des Sciences* for 1740, p. 586 and 604. Among other remarkable singularities in this little monstrous abortion of 6 months, that excellent anatomist particularly takes notice, that there was no appearance of one drop of red blood in any of its vessels, which were universally filled with a serous lymph; and that there were no vestiges of any veins at all.—Orig.

§ F. Licetus, de *Monstris*, p. 300 et seq. Palfyn, *Traite des Monstres*, p. 325. Cheselden's *Anatomy*, 5th ed. p. 379. *Phil. Trans.*, 1739-40, N° 456. *Ibid.* 1767, p. 1. *L'Academie des Sciences*, Hist. 1720, p. 13. *Ibid.* Mem. 1720, p. 8. *Ibid.* 1740, p. 586 and p. 592. *Miscellanea Curiosa Ephemeridum Germanicarum Ann.* 19, p. 258. *Acta Eruditorum Lipsiæ*, Ann. 1724, p. 501.—Orig.

affords also an exception to a frequent remark among authors, "That brainless children are always very brisk before they are born;"\* for the mother has frequently said, "That she felt no motion at all within her after the first birth; and that she had not the least suspicion of there being a 2d child till it was delivered." This circumstance may however perhaps be attributed to the medulla spinalis being totally deficient, as well as the cerebrum and cerebellum.

Physiologists and philosophers have spent a great deal of time in attempting to investigate the causes of these extraordinary phenomena. With this view many opinions have been started; but most, if not all of them, as far as Dr. C. was able to judge, being built on the tottering basis of conjecture only, afford, on an attentive inspection, but little satisfaction to a dispassionate inquirer after truth. The particular hypothesis, which has been almost universally adopted, is, that monstrosity and marks in children depend on the imagination and longing of the mother. Such a pernicious principle as this ought to have very rational evidence, and the most striking facts to support it. But is it not directly the contrary? Indeed a great many ridiculous stories have been related to the world,† which however on a little reflection either obviate themselves, or else are contradicted by those facts that occur. May we not exemplify this observation by the case of twins now related? One of the children was perfect, and is still living; the other proves to be remarkably defective. Does not the question naturally arise here, how could one child be affected by the disturbed imagination of the mother, and the other not? But the mother, on repeated examination, recollects no fright in particular while she was pregnant. Neither, if she did, would it at all invalidate the force of our argument on this subject; for she could not possibly see any child without a head: and more especially, because other parts, as the viscera and medulla spinalis, were equally defective, which are entirely out of the reach of the eye or imagination of the mother to form any idea about them. To elucidate this point still further, can any candid person possibly suppose, that the casual agitation of mind of a pregnant woman, should either produce or destroy a whole system of blood vessels, nerves, and fibres, which are indispensable constituents of almost every part of the body? And may we not adduce one proof more, in support of our argument, from what happens to animals and vegetables? Among these also, such extraordinary deviations from the general course of nature are by no means uncommon: yet the former are possessed of a much less share of imagination than is generally allotted to the human species; and the latter have none at all. Reasoning in

\* Phil. Trans. 1674, No. 99, p. 6157. Ibid. 1767, p. 18.—Orig.

† Mauriceau, p. 53, obs. 64. Ibid. p. 63, obs. 118. Smellie's Midwifry, vol. 3, p. 402. Phil. Trans., 1684, N<sup>o</sup> 160, p. 599. Ibid. 1739-40, N<sup>o</sup> 456, p. 303 and 306.—Orig.

the same manner on several occasions of this kind in which he had been concerned, his conclusions had always been similar; viz. that the usually assigned cause of the mother's imagination is by no means equal to the manifold effects produced. And on the other hand, this injurious doctrine is pregnant with continual mischief to society. It frequently makes women very unhappy. And the fear of mutilating or marking their infants often affects them so much, that they at last miscarry. Having therefore indubitable facts to go on, and the cause of humanity so powerfully coinciding with the truth, is it not right to affirm and maintain with confidence, that neither the longing nor frightened imagination of the mother appears to have any power at all to imprint marks or monstrosity on children? That this is a very weak supposition, entirely void of foundation, directly contrary to all philosophy and experience, and has nothing to support it but a vulgar opinion, transmitted to us from the ages of anatomical ignorance? And is it not more reasonable to conclude, as Dr. Hunter in his lectures has done, that whatever be the defect or deformity in a monstrous birth, it can never be occasioned by accidents of any kind during pregnancy; but probably has its existence always originating, *causâ adhuc incognitâ*, in the first stamina of the embryo.\*

Thus have been faithfully related the particulars of this singular phenomenon among the human species, which, to a demonstration, confirm Dr. Hunter's opinion, that the nourishment of the foetus in utero is principally by means of the funis umbilicalis. M. Merry observes, that defective monsters are more instructive than others that have redundancies.† If this be true, here is still an ample field for speculation, notwithstanding the few very obvious remarks which Dr. C. already ventured to make. In conformity to the general language of authors, he had in this essay occasionally adopted the use of the term monster: there is however something in that word extremely repugnant to our common feelings, and very apt to leave a terrifying impression on the mind. Why may not the Author of Being sometimes produce variations in the human species, as well as in the animal and vegetable kingdoms,‡ and equally exempt too from such frightful appellations? Would it not therefore be more eligible in the present instance, and every similar one, to explode the common term, and call it simply a *lusus naturæ*; or with Pliny to say, "*Hoc nobis miraculum, sibi ludibrium, ingeniosa finxit natura.*"

\* Baron Haller is of opinion also, that this is evidently the case in that species of monsters to which parts are added. Vide *Opera Minora Halleri*, tom. 3, p. 148.—Orig.

† L'Académie des Sciences, Hist. 1720, p. 13.—Orig.

‡ See F. Licetus, J. Palfyn des Monstres, &c. in which are many instances of each kind.—Orig.

*XXXI. Observations on the State of Population in Manchester, and other adjacent Places, concluded. By Thomas Percival, M. D., F. R. S., and S. A. p. 322.*

Reprinted in Dr. Percival's collected works recently, 1807, edited by his son, in 4 vols. 8vo. We shall therefore only observe here, that from these tables it appears, that the proportion of males to females baptized, is nearly as 12 to 11 $\frac{1}{3}$ , or 19 to 18; but that the number of females living is to the number of males as about 11 to 10 $\frac{1}{3}$ , or more exactly as 14 to 13; and that the widows are almost double the number of widowers.

*XXXII. On the Effects of Lightning on a House, which was furnished with a Pointed Conductor, at Tenterden, in Kent. In Two Letters from Richard Haffenden, Esq., the Proprietor of the House, to Mr. Henley. To which are added some Remarks by Mr. Henley. p. 336.*

This was an oblong house, about 50 feet long and 30 broad, having 4 stacks of chimnies, 2 at each end, to one stack of which was fixed a pointed iron conductor, projecting 5 feet above the top of the chimney. The lightning struck the chimney diagonally opposite to and the farthest from that of the conductor, and in its passage injured several parts of the building, where the conductors were imperfect or discontinued. On the account given of the house and the accident, &c. Mr. Henley remarks that, 1st, A sharp pointed conductor did not, in this instance, invite or draw down on itself a stroke of lightning. 2dly, Such a conductor, elevated 5 feet above the top of the chimnies, to a house of this dimension, may not perhaps be sufficient, by its silent attractive force, to protect the whole of such a building from a stroke; especially when a chimney, a blunt body, wetted with the rain, standing at 50 feet distance from the conductor, and being within 5 feet of its height, is in actual contact with so large, though irregular, a communication of metal, leading from the chimney directly to the conductor; though, in this instance, it should be-remarked, that the conductor itself was not in contact throughout; and it is, for that reason, a very exceptionable case. 3dly, Two such conductors; one, for instance, on the chimney where this was placed; and the other on the chimney which was stricken, with a communication of lead between them, would probably have protected the house: but a conductor on each chimney would certainly have secured the whole building effectually. 4thly, As the 3 branches, or divisions of the lightning, all concentrated on an iron bar, three quarters of an inch square, and produced no sign of heat in it, an iron bar of that size seems to be fully sufficient for the purpose. There appears however to have been 2 defects in Mr. Haffenden's conductor: 1. The leaden pipe and the iron bar at the bottom were not in contact. 2. The iron bar, or a thick plate of lead,

should have been continued down into the moist earth or water; and had not the earth, as Mr. Haffenden observes, flowed with water, at the time of the accident, the want of this precaution might perhaps have been attended with some damage to the foundation. In Mr. Haffenden's 2d letter, he observes, that the bell wire, mentioned in his first letter, was brass; and that so much of it as went through the passage painted: and the painted part, he says, was not destroyed; but the paint was loosened on the wire, without being broken off, like the loose rind of a tree; which resembles the effect of the artificial electricity, in an experiment of Mr. Kinnersley's, where a wire was, by a great explosion, both lessened in diameter, and extended in length. The other part of the wire, which was not painted, except a short piece at the end, somewhat larger and of iron, was entirely melted. Query, if the wire before spoken of had passed through a stone, particularly a wet one, inclosing it firmly, would not that stone have been shivered to pieces?

*XXIII. On the Torpidity of Swallows and Martins. By James Cornish, Surgeon, Totness, Devon. p. 343.*

In the beginning of November, Mr. C. being fishing on the banks of the river Dart, which runs at the bottom of a very steep hill, from the side of which project several large rocks, overgrown with ivy and thicket; he was at once surprised with the sight of a great number of martins. He desisted from his amusement, the more carefully to observe the birds, which he concluded had been brought out of their winter quarters by the fineness of the afternoon, it being remarkably pleasant and warm for the time of the year; the sun at that time darting its rays directly against the rocks, just opposite to which Mr. C. had fixed his station. They continued to flit to and fro for near half an hour, keeping very near together, and never flying in a direct line above 30 or 40 yards, and never, when at the farthest, above 100 yards distant from the rocks; closer to which they now, as the sun lowered, began to gather very fast. Their numbers now lessened considerably; and in a very short time they all returned into the fissures of the rocks, whence they had been induced to venture out by the warmth of the evening. Mr. C. was particularly careful to observe if there was a swallow among them; but there was not one. Of this he was certain; for they were several times within the distance of 20 yards from the places where he stood. He was the more attentive to this, as he had been repeatedly assured, by many masters of vessels in the fish trade, that they constantly saw every autumn, as they sailed up the Mediterranean, vast flights of swallows, bending their course towards the south. From which there is the strongest reason to believe, that these birds seek a warmer climate during the winter months;

though Mr. Buffon has left that point undetermined. The above account Mr. C. thinks settles the question, relative to martins.

*XXXIV. Description and Use of a portable Wind Gage. By James Lind, M. D., Edinburgh.\* p. 353.*

This simple instrument consists of 2 glass tubes AB, CD, of 5 or 6 inches in length, pl. 13, fig. 4. Their bores, which are so much the better always for being equal, are each about  $\frac{1}{10}$  of an inch in diameter. They are connected together, like a siphon, by a small bent glass tube ab, the bore of which is  $\frac{1}{10}$  of an inch in diameter. On the upper end of the leg AB there is a tube of latten brass, which is kneed or bent perpendicularly outwards, and has its mouth open towards F. On the other leg CD is a cover, with a round hole G in the upper part of it,  $\frac{1}{10}$  of an inch in diameter. This cover and the kneed tube are connected together by a slip of brass cd, which not only gives strength to the whole instrument, but also serves to hold the scale HI. The kneed tube and cover are fixed on with hard cement or sealing wax. To the same tube is soldered a piece of brass e, with a round hole in it, to receive the steel spindle KL, and at f there is just such another piece of brass soldered to the brass hoop gh, which surrounds both legs of the instrument. There is a small shoulder on the spindle at f, on which the instrument rests, and a small nut at i, to prevent it from being blown off the spindle by the wind. The whole instrument is easily turned round on the spindle by the wind, so as always to present the mouth of the kneed tube towards it. The end of the spindle has a screw on it; by which it may be screwed into the top of a post, or a stand made on purpose. It has also a hole at L, to admit a small lever for screwing it into wood with more readiness and facility. A thin plate of brass k is soldered to the kneed tube, about half an inch above the round hole G, so as to prevent rain from falling into it. There is likewise a crooked tube AB, fig. 5, to be put occasionally on the mouth of the kneed tube F, in order to prevent rain from being blown into the mouth of the wind gage, when it is left out all night, or exposed in the time of rain.

The force or momentum of the wind may be ascertained by the assistance of this instrument, by filling the tubes half full of water, and pushing the scale a little up or down, till the O of the scale, when the instrument is held up perpendicularly, be on a line with the surface of the water, in both legs of the wind-gage. The instrument being thus adjusted, hold it up perpendicularly, and turning the mouth of the kneed tube towards the wind, observe how much the water is depressed by it in the one leg, and how much it is raised in the other. The sum of the two is the height of a column of water which the wind is capable of sustaining at that time; and every body that is opposed to that

\* Now of Windsor.

wind, will be pressed on by a force equal to the weight of a column of water, having its base equal to the surface that is opposed, and its height equal to the altitude of the column of water sustained by the wind in the wind gage. Hence the force of the wind on any body, where the surface opposed to it is known, may be easily found; and a ready comparison may be made between the strength of one gale of wind and that of another, by knowing the heights of the columns of water, which the different winds were capable of sustaining. The heights of the columns in each leg will be equal, provided the legs are of equal bores; but unequal, if their bores are unequal. For suppose the legs equal, and the column of water the wind sustains to be 3 inches, the water in the leg, which the wind blows into, will be depressed  $1\frac{1}{4}$  inch below 0, and raised just as much above it in the other leg. But if the bore of the leg which the wind blows into, be double that of the other, the water in that leg will be depressed only 1 inch, while it is raised twice as much, or 2 inches, in the other; and vice versa, if the same wind blows into the smaller leg, it will depress the water in it 2 inches, while it raises it only 1 inch in the other. The force of the wind may be likewise measured with this instrument, by filling it till the water runs out of the hole G. For if we then hold it up to the wind as before, a quantity of water will be blown out; and, if both legs of the instrument are of the same bore, the height of the column sustained, will be equal to double the column of water in either leg, or the sum of what is wanting in both legs. But if the legs are of unequal bores, neither of these will give the true height of the column of water which the wind sustained. But the true height may be obtained by the following formulæ.

Suppose that after a gale of wind, which had blown the water in one of the tubes from A to B, fig. 6, forcing it at the same time through the other tube out at E, the surface of the water should be found standing at some level DG, and it were required to know what was the height of the column EF or AB, which the wind sustained. In order to obtain which, it is only necessary to find the height of the columns DB or GF, which are constantly equal to each other: for either of these added to one of the equal columns AD, EG, will give the true height of the column of water which the wind sustained.

*Case 1.* Let the diameters AC, EH, of the tubes be respectively represented by  $c$ ,  $d$ ; and let  $a = AD$  or  $EG$ , and  $x = DB$  or  $GF$ . Then it is evident that the column DB, is to the column EG, as  $c^2x$  to  $d^2a$ . But these columns are equal. Therefore  $c^2x = d^2a$ ; and consequently  $x = \frac{d^2a}{c^2}$ .—Example. If the diameters AC, EH, be respectively 10 and 1, and  $AD$  or  $EG = 3.96$  inches,  $x$  will be  $= .0396$  of an inch. For  $d^2a = 1 \times 3.96 = 3.96$ , which divided by  $c^2 = 100$ , gives  $x = .0396$ .

*Case 2.* But if at any instant of time, while the wind was blowing, it was

observed, that when the water stood at E, the top of the tube out of which it is forced, it was depressed in the other tube to some given level BF, the altitude at which it would have stood in each, had it immediately subsided, may be found in the following manner. Let  $b = AB$  or  $EF$ . Then it is evident, that the column DB is equal to the difference of the columns EF, GF. But the difference of these columns is as  $d^2b - d^2x$ . Therefore  $c^2x = d^2b - d^2x$ ; and consequently,  $x = \frac{d^2b}{c^2 + d^2}$ .

For the cases when the wind blows in at the narrow leg of the instrument. Let  $AB = EF = b$ ,  $EG$  or  $AD = a$ ,  $GF = DB = x$ , and the diameters EH, CA, respectively  $= d, c$ , as before. Then it is evident that the column AD, is to the column GF, as  $ac^2$  to  $d^2x$ . But these columns are equal. Therefore  $d^2x = ac^2$ ; and consequently  $x = \frac{ac^2}{d^2}$ . This answers to case 1. It is also evident, that the column AD is equal to the difference of the columns AB, DB. But the difference of these columns is as  $bc^2 - c^2x$ . Therefore  $d^2x = bc^2 - c^2x$ . Whence we get  $x = \frac{bc^2}{d^2 + c^2}$ . This corresponds to case 2.

As there is always a calculation to be made for every experiment when the legs of the instrument are of unequal bores, Dr. L. recommends it to the makers of these instruments to choose tubes that are equal, or at least nearly so, that the error may become next to nothing, it being a thing very easy to be done. In this manner we can readily determine the greatest force, with which the wind has blown during the time the instrument has been exposed to its action. But as it may be safely left alone, by screwing its spindle into the proper stand, or into the top of a post, and as the wind never fails to turn the mouth of it towards itself, it is not necessary for the observer to continue always by it; for it may be allowed to stand all night, exposed to the wind, without any inconvenience, though it should even happen to rain very heavily. However, recourse can only be had to this method of using the instrument on shore; for at sea it must always be held up in a perpendicular position in the hand, whether it be used when only half full of water, or when quite full; which last will be frequently found to be the only practicable method of ascertaining the force of the wind during the night, when it blows so hard that it is impossible to keep any lights on deck.

A person filling the wind-gage, in a calm place, with water, in order to determine the force of the wind, in the way just described, will be apt to imagine, that it cannot give the measurement correct; for he will find such a repulsion to arise from the edges of the hole G, as to sustain a column of water in the kneed or bent tube, perhaps half an inch above the level; but by either blowing across the round hole, or moving his finger over it, he will soon bring the water in the kneed tube to stand at the same level with it, by taking off gradually the convex surface of the water, which projects out at the hole in the form of a



drop or spherule; and this effect the wind very soon produces itself. There ought always to be a cover on the top of the tube, out of which the water is expelled by the wind; but it should be made very thin. For if there be no such cover, and the mouth of the kneed tube be stopped, after the instrument is quite full of water, in order to prevent the wind from having any influence in raising it, you will find, on exposing it to a strong gale, that in a very short time it will blow out perhaps half an inch of water. Whence it appears, that a very considerable error would arise from using the wind-gage in this state. The use of the small tube of communication *ab* (fig. 4) is to check the undulation of the water, so that its height may be read off from the scale with ease and certainty. But it is particularly designed to prevent the water from being thrown up to a much greater or less altitude, than the true height of the column, which the wind is able at that time to sustain, from its receiving a sudden impulse, while it is vibrating either in its ascent or descent. For water in the legs of a siphon is capable of being put into a vibrating motion like a pendulum; and therefore, if acted on when in the ascent, the height which it ascends to will come out greater than the truth, and less, if acted on in the descent.

The height of the column of water sustained in the wind-gage being given, the force of the wind on a foot square is easily had by the following table, and consequently on any known surface.

TABLE I.

Height of the water in the wind-gage.	Force of the wind on the foot square in avoirdupois pounds.	Common designation of such a wind.
12 inches .....	62.5	
11 .....	57.293	
10 .....	52.083	} most violent hurricane.
9 .....	46.875	
8 .....	41.657	.. very great ditto.
7 .....	36.548	.. great hurricane.
6 .....	31.75	.. hurricane.
5 .....	26.041	.. very great storm.
4 .....	20.833	.. great ditto.
3 .....	15.625	.. storm.
2 .....	10.416	.. very high wind.
1 .....	5.208	.. high wind.
0½ .....	2.604	.. brisk gale.
0⅓ .....	.521	.. fresh breeze.
0⅔ .....	.260	.. pleasant wind.
0¼ .....	.130	.. a gentle wind.

answer the purpose, as also subjoins a general method of reducing them all to one common measure. But of all the fluids he was acquainted with, when the effects of frost are to be feared, he knew none better adapted to the purpose than a saturated solution of sea-salt; since it does not freeze till the thermometer falls to 0 degrees, and is a fluid constantly of the same specific gravity. Spirit of wine, independent of its being more variable in respect of specific gravity by the influ-

It may be sometimes necessary to employ other fluids besides water, particularly if the degree of cold be below freezing; for then we must use a fluid that will not freeze in the degree of cold in which we expose the instrument, otherwise the wind can have no influence on it, and the liquor freezing in the tube will break it. Dr. L. therefore mentions a few liquors in the following table that will an-

ence of heat and cold, is also more or less so, as it is more or less rectified. And though the true specific gravity were known at the beginning of the operation, it would even change during the time of using it, by imbibing moisture from the air.

*Example.* If it were required to know the force of the wind, when the column of water sustained was equal to  $4\frac{6}{10}$  inches. Then, by tab. 1,

4 inches = . . . . 20.833 lb.

0.5 or  $\frac{1}{2}$  inch = 2.604

$$0.1 = \dots\dots\dots 0.521$$

$$\text{sum } 4.6 = \dots\dots\dots 23.958 = \text{force on every square foot.}$$

TABLE II.

Names of liquors.	Specific gravities.	Common multiplier.	Weight Measuring the forces of the wind.
Water.....	1.000	} nw }	1 × nw
Sat sol. of salt..	1.244		1.244 × nw
Urine.....	1.030		1.030 × nw
Ditto.....	1.016		1.016 × nw
Alkohol.....	0.825		0.825 × nw
Proof spirits ..	0.927		0.927 × nw
&c. &c.			&c. &c.

Let  $w$  represent the weight of a column of water, having its altitude measured by one of the divisions on the scale, and its base equal to any given surface whatever; and let  $n$  denote in general the number of these divisions that measures

the whole length of the column of the water which the wind sustains. Then  $mw$  will represent always its weight, and will serve as a common multiplier for the specific gravities of all other liquors.

*Example.* Let  $w$  represent the weight of a column of water  $\frac{1}{10}$  of an inch high, standing on a square foot; and let  $n = 80 = 4$  inches. Then, by tab. 1,  $nw$  is equal to 20.833 avoirdupois pounds. Therefore  $1.244 \times 20.833 =$  weight of a saturated solution of sea-salt of the same altitude, and  $\frac{4}{1.244} =$  the altitude of a column of a saturated solution of the same, weighing 20.833 lb. avoirdupois;  $w$  may represent a square yard, the surface of a sail, &c.

If the velocity and density of the wind in any particular case were accurately determined, this instrument, which gives its force or momentum, would enable us to ascertain the velocity in every other case, the density being known. For it appears from experiments, made by Mr. James Ferguson, F.R.S., on the whirling table, that its force is as the square of its velocity. But as the density, which is one of the data requisite for determining the velocity of this instrument, was not taken into consideration in these experiments, all that we can do at present is to suggest the idea.

p. 8.—The wind-gage ought to be somewhat longer than that first mentioned. For they had a gale at Edinburgh May 9th, 1775, which supported a column of water of  $6\frac{7}{10}$  inches. The force of this gale on a square foot was equal to 34.921 lb avoirdupois, and it did great damage to the gardens. West India hurricanes would require gages of a still greater length to measure them.

*XXXV. Astronomical Observations made at Leicester. By the Rev. Mr. Ludlam, Vicar of Norton, near Leicester. p. 366.*

The first observations are a set of zenith distances of stars, to determine the

latitude of the place; which comes out  $52^{\circ} 38'$  very exactly. Next follow a few observed occultations; and some solar transits, to examine the clock.

*XXXVI. Remarks and Considerations relative to the Performance of Amputation above the Knee, by the Single Circular Incision. By Benjamin Gooch, Surgeon, at Norwich. p. 273.*

Reprinted in Mr. Gooch's Chirurgical Works, 3 vols. 8vo., 1792.

*XXXVII. Concerning Aneurysms in the Thigh. By Benjamin Gooch, Surgeon, at Norwich. p. 378.*

May be consulted in Mr. Gooch's Chirurgical Works above referred to.

*XXXVIII. An Account of further Discoveries in Air. By the Rev. Joseph Priestley, LL. D., F. R. S. p. 384.*

Reprinted in Dr. Priestley's collected works on different Kinds of Air.

*XXXIX. An Account of the Gymnotus Electricus. By John Hunter, F. R. S. p. 395.*

To Mr. Walsh, the first discoverer of animal electricity, the learned will be indebted for whatever the following pages may contain, either curious or useful. The specimen of the animal which they describe was procured by that gentleman, and at his request this dissection was performed, and this account of it is communicated.

This fish, on the first view, appears very much like an eel, from which resemblance it has most probably got its name; but it has none of the specific properties of that fish. This animal may be considered, both anatomically and physiologically, as divided into 2 parts; viz. the common animal part; and a part which is superadded, viz. the peculiar organ. I shall at present consider it only with respect to the last; as the first explains nothing relating to the other, nor any thing relating to the animal economy of fish in general. The first, or common animal part, is so contrived as to exceed what was necessary for itself, in order to give situation, nourishment, and most probably the peculiar property to the second. The last part, or peculiar organ, has an immediate connection with the first; the body affording it a situation; the heart, nourishment; and the brain, nerves, and probably its peculiar powers. For the first of these purposes, the body is extended out in length, being much longer than would be sufficient for what may be called its progressive motion. For the real body, or that part where the viscera and parts of generation lie, is situated, with respect to the head, as in other fish, and is extremely short; so that, according to the ordinary proportions, this should be a very short fish. Its great length, therefore, seems chiefly intended to afford a surface for the support of the peculiar

organ: however, the tail part is likewise adapted to the progressive motion of the whole, and to preserve the specific gravity; for the spine, medulla spinalis, muscles, fin, and air bladder, are continued through its whole length. Besides which parts, there is a membrane passing from the spine to that fin which runs along the belly or lower edge of the animal. This membrane is broad at the end next the head, terminating in a point at the tail. It is a support for the abdominal fin, gives a greater surface of support for the organ, and makes a partition between the organs of the two opposite sides.

*The Organs.*—The organs which produce the peculiar effect of this fish, constitute nearly one-half of that part of the flesh in which they are placed, and perhaps make more than one-third of the whole animal. There are 2 pair of these organs, a larger, and a smaller; one being placed on each side. The large pair occupy the whole lower or anterior, and also the lateral part of the body, making the thickness of the fore or lower parts of the animal, and run almost through its whole length; viz. from the abdomen to near the end of the tail. It is broadest on the sides of the fish at the anterior end, where it incloses more of the lateral parts of the body, becomes narrower towards the end of the tail, occupying less and less of the sides of the animal, till at last it ends almost in a point. These two organs are separated from one another at the upper part, by the muscles of the back, which keep their posterior or upper edges at a considerable distance from one another; below that, and towards the middle, they are separated by the air bag; and at their lower parts they are separated by the middle partition. They begin forwards, by a pretty regular edge, almost at right angles with the longitudinal axis of the body situated on the lower and lateral parts of the abdomen. Their upper edge is a pretty straight line, with small indentations made by the nerves and blood vessels, which pass round it to the skin. At the anterior end they go as far towards the back as the middle line of the animal; but in their approach towards the tail they gradually leave that line, coming nearer to the lower surface of the animal. The general shape of the organ, on an external or side view, is broad at the end next the head of the animal, becoming gradually narrower towards the tail, and ending there almost in a point. The other surfaces of the organ are fitted to the shape of the parts with which they come in contact; therefore on the upper and inner surface it is hollowed, to receive the muscles of the back. There is also a longitudinal depression on its lower edge, where a substance lies, which divides it from the small organ, and which gives a kind of fixed point for the lateral muscles of the fin. Its most internal surface is a plane adapted to the partition which divides the two organs from one another. The edge next the muscles of the back is very thin, but the organ becomes thicker and thicker towards its middle, where it approaches the centre

of the animal. It becomes thinner again, towards the lower surface or belly; but that edge is not so thin as the other. Its union with the parts to which it is attached is in general by a loose, but pretty strong, cellular membrane; except at the partition, to which it is joined so close as to be almost inseparable.

The small organ lies along the lower edge of the animal, nearly to the same extent as the other. Its situation is marked externally by the muscles which move the fin under which it lies. Its anterior end begins nearly in the same line with the large organ, and just where the fin begins. It terminates almost insensibly near the end of the tail, where the large organ also terminates. It is of a triangular figure, adapting itself to the part in which it lies. Its anterior end is the narrowest part; towards the tail it becomes broader; in the middle of the organ it is thickest; and from thence becomes gradually thinner to the tail, where it is very thin. The two small organs are separated from one another by the middle muscle, and by the bones on which the bones of the fins are articulated. The large and the small organ on each side, are separated from one another by a membrane, the inner edge of which is attached to the middle partition, and its outer edge is lost on the skin of the animal. To expose the large organ to view, nothing more is necessary than to remove the skin, which adheres to it by a loose cellular membrane. But to expose the small organ, it is necessary to remove the long row of small muscles which move the fin.

*Of the structure of these Organs.*—The structure is extremely simple and regular, consisting of 2 parts; viz. flat partitions or septa, and cross divisions between them. The outer edge of these septa appear externally in parallel lines nearly in the direction of the longitudinal axis of the body. These septa are thin membranes, placed nearly parallel to one another. Their lengths are nearly in the direction of the long axis, and their breadth is nearly the semidiameter of the body of the animal. They are of different lengths, some being as long as the whole organ. I shall describe them as beginning principally at the anterior end of the organ, though a few begin along the upper edge; and the whole, passing towards the tail, gradually terminate on the lower surface of the organ; the lowermost at their origin terminating soonest. Their breadths differ in different parts of the organ. They are in general broadest near the anterior end, answering to the thickest part of the organ, and become gradually narrower towards the tail, however they are very narrow at their beginnings or anterior ends. Those nearest the muscles of the back are the broadest, owing to their curved or oblique situation on these muscles, and get gradually narrower towards the lower part, which is in a great measure owing to their becoming more transverse, and also to the organ becoming thinner at that place. They have an outer and an inner edge. The outer is attached to the skin of the animal, to the lateral muscles of the fin, and to the membrane which divides

the great organ from the small; and the whole of their inner edges are fixed to the middle partition formerly described, also to the air bladder, and 3 or 4 terminate on that surface which incloses the muscles of the back. These septa are at the greatest distance from one another at their exterior edges near the skin, to which they are united; and as they pass from the skin towards their inner attachments, they approach one another. Sometimes we find 2 uniting into one. On that side next the muscles of the back, they are hollow from edge to edge, answering to the shape of those muscles; but become less and less so towards the middle of the organ; and from that towards the lower part of the organ, they become curved in the other direction. At the anterior part of the large organ, where it is nearly of an equal breadth, they run pretty parallel to one another, and also pretty straight; but where the organ becomes narrower, it may be observed in some places, that 2 join or unite into 1; especially where a nerve passes across. The termination of this organ at the tail is so very small that I could not determine whether it consisted of one septum or more. The distances between these septa will differ in fishes of different sizes. In a fish of 2 feet 4 inches in length, I found them to be about  $\frac{1}{17}$  of an inch distant from one another; and the breadth of the whole organ, at the broadest part, about an inch and a quarter, in which space were 34 septa. The small organ has the same kind of septa, in length passing from end to end of the organ, and in breadth passing quite across; they run somewhat serpentine, not exactly in straight lines. Their outer edges terminate on the outer surface of the organ, which is in contact with the inner surface of the external muscle of the fin, and their inner edges are in contact with the centre muscles. They differ very much in breadth from one another; the broadest being equal to one side of the triangle, and the narrowest scarcely broader than the point or edge. They are pretty nearly at equal distances from one another; but much nearer than those of the large organ, being only about  $\frac{1}{16}$  part of an inch asunder: but they are at a greater distance from one another towards the tail, in proportion to the increase of breadth of the organ. The organ is about half an inch in breadth, and has 14 septa. These septa, in both organs, are very tender in consistence, being easily torn. They appear to answer the same purpose with the columns in the torpedo, making walls or buttments for the sub-divisions, and are to be considered as making so many distinct organs. These septa are intersected transversely by very thin plates or membranes, whose breadth is the distance between any 2 septa, and therefore of different breadths in different parts; broadest at the edge which is next to the skin; narrowest at that next to the centre of the body, or to the middle partition which divides the 2 organs from one another. Their lengths are equal to the breadths of the septa, between which they are situated. There is a regular series of them continued from one end of any 2

septa to the other. They appear to be so close as even to touch. In an inch in length there are about 240, which multiplies the surface in the whole to a vast extent.

*Of the Nerves.*—The nerves in this animal may be divided into 2 kinds; the 1st, appropriated to the general purposes of life; the 2d, for the management of this peculiar function, and very probably for its existence. They arise in general from the brain and medulla spinalis, as in other fish; but those from the medulla are much larger than in fish of equal size, and larger than is necessary for the common operations of life. The nerve which arises from the brain, and passes down the whole length of the animal (which I believe exists in all fish) is larger in this than in others of the same size, and passes nearer the spine. In the common eel it runs in the muscles of the back, about midway between the skin and spine. In the ood it passes immediately under the skin. From its being larger in this fish than in others of the same size, one might suspect, that it was intended for supplying the organ in some degree; but this seems not to be the case, as I was not able to trace any nerves going from it to join those from the medulla spinalis, which run to the organ. This nerve is as singular an appearance as any in this class of animals; for surely it must appear extraordinary, that a nerve should arise from the brain to be lost in common parts, while there is a medulla spinalis giving nerves to the same parts. It must still remain one of the inexplicable circumstances of the nervous system. The organ is supplied with nerves from the medulla spinalis from which they come out in pairs between all the vertebræ of the spine. In their passage from the spine they give nerves to the muscles of the back, &c. They bend forwards and outwards on the spine, between it and the muscles, and send out small nerves to the external surface, which join the skin near to the lateral lines. These ramify on the skin, but are principally bent forwards between it and the organ, into which they send small branches as they pass along. They seem to be lost in these 2 parts. The trunks get upon the air-bladder, or rather dip between it and the muscles of the back, and continuing their course forwards on that bag, they dip in between it and the organ, where they divide into smaller branches, they then get upon the middle partition, on which they continue to divide into still smaller branches; after which they pass on, and get upon the small bones and muscles, which are the bases for the under fin, and at last they are lost on that fin. After having got between the organ and the abovementioned parts, they are constantly sending small nerves into the organs; first into the great organ, and then into the small one; also into the muscles of the fin, and at last into the fin itself. These branches, which are sent into the organ as the trunk passes along, are so small, that I could not trace their ramifications in the organs. In this fish, as well as in the torpedo, the nerves which supply the organ are much larger than those

bestowed on any other part for the purposes of sensation and action ; but it appears to me, that the organ of the torpedo is supplied with much the largest proportion. If all the nerves which go to it were united together, they would make a vastly greater chord, than all those which go to the organ of this eel. Perhaps when experiments have been made on this fish, equally accurate with those made on the torpedo, the reason for this difference may be assigned.

*Blood Vessels.*—How far this organ is vascular, I cannot positively determine ; but from the quantities of small arteries going to it, I am inclined to believe, that it is not deficient in vessels. The arteries arise from the large artery which passes down the spine ; they go off in small branches like the intercostals in the human subject, pass round the air bladder, and get upon the partition together with the nerves, and distribute their branches in the same manner. The veins take the same course backwards, and enter the large vein which runs parallel with the artery.

Pl. 14, fig. 1, Shows the whole animal of  $\frac{1}{4}$  the full size. It lies on one side ; which posture exposes the whole of the under fin. The head is twisted, to show its upper part, on which are seen the eyes, &c.

Fig. 2, Shows the animal lying in the same position, but the head is twisted in the contrary direction, so as to expose its under surface. Between the two fins, and before the beginning of the under fin, is the cavity of the belly of the fish ; at the anterior part of which cavity is the anus.

Fig. 3, Exhibits the whole of the two organs on each side, the skin being removed as far as these organs extend. A The lower surface of the head of the animal ; B, the cavity of the belly, C, the anus ; D, the fin ; E, the back of the fish where the skin has not been removed ; FF, the fin which runs along the lower edge of the fish ; GGG, the skin turned back ; HHH, the lateral muscles of the above fin removed and carried back with the skin, to expose the small organ. 1. Part of the muscle left in its place. KKK, the large organ ; LLL, the small organ ; MMMM, the substance which divides the large organ from the small ; N, at this place the above substance is removed.

Fig. 4, Is a section of the whole thickness of the fish near the upper part. The skin is removed as far back as the posterior edge of the organ, and the other parts immediately belonging to it, such as the medulla spinalis. There are several pieces or sections taken out of the organ, which expose every thing that has any relation to it. At the upper and lower ends of the figure, FF, the organ is entire, the skin only being removed. AA, The body of the animal near the back, covered by the skin ; BB, the belly-fin, covered also by the skin ; C, part of the skin removed from the organ, and turned back ; DD, the muscles which move the fin laterally, and which immediately cover the small organ ; E, the middle muscles of the fin, which lay immediately between the two small organs ; FF, the outer surface of the large organ, as it appears when the skin is removed ; G, the small organ, as it appears when the lateral muscles are removed ; H H, the cut ends of the muscles of the back, which have been removed to expose the deeper seated parts ; I I, the cut ends of the large organ, part of which has also been removed, to expose the deeper seated parts ; K, the cut end of the small organ ; L, a part of the large organ, the rest having been removed ; M, the cut end of the above section ; N, a section of the small organ ; O O, the middle partition which divides the two large organs ; P, a fatty membrane, which divides the large organ from the small ; Q, the air bladder ; R, the nerves going to the organ ; S, the medulla spinalis ; T, the singular nerve.

Fig. 5, is a transverse section of the fish, exposing at one view, all the parts of which it is composed ; A, the external surface of the side of the fish ; B, the under fin ; CCCC, the cut ends of the



muscles of the back; D, the cavity of the air bladder; E, the body of the spine; F, the medulla spinalis; G, the large artery and vein; HH, the cut ends of the two large organs; II, the cut ends of the two small organs; K, the partition between the organs.

*XL. Some Observations on Myrrh,\* made in Abyssinia, in 1771, and sent to Wm. Hunter, M. D., with Specimens, in Feb. 1775. By James Bruce,† Esq. p. 408.*

The ancients, and particularly Dioscorides, spoke of myrrh in such a manner as to make us suppose, either that they have described a drug which they had never seen; or that the drug seen and described by them is absolutely unknown to modern naturalists and physicians. The Arabs, however, who form the link of the chain between the Greek physicians and ours, in whose country the myrrh was produced, and whose language gave it its name, have left us undeniable evidence, that what we know by the name of myrrh, is in nothing different from the myrrh of the ancients, growing in the same countries from which it was brought formerly to Greece; that is, from the east coast of Arabia Felix, bordering on the Indian Ocean, and that low land in Abyssinia on the south-east of the Red Sea, included nearly between the 12th and 13th degree of north latitude, and limited on the west by a meridian passing through the island Massowa, and on the east by another passing through Cape Guardsoy, without the straits of Babelmandel. This country the Greeks knew by the name of

\* Some years after this paper was sent to the R. S. Mr. Bruce gave a farther account of myrrh in the 5th vol. of his travels to discover the source of the Nile, whence it would appear that the plant, from which the genuine myrrh is obtained, is a species of mimosa.

† This celebrated Abyssinian traveller was born at his paternal estate at Kinnaird, near Falkirk, in Dec. 1729, and where he died in 1794, in the 65th year of his age. He received the first rudiments of his education at Harrow school, which he finished in his own country. An early propensity to travel induced him to seek an appointment in India; but being disappointed in that, he engaged in the wine-trade in London; soon after, however, he visited several parts of Europe; but on the death of his father, he returned to Britain, in 1761. Being offered the consulship of Algiers, he accepted that office, as affording an opportunity of gratifying his roving disposition. Accordingly in 1763 he set out for that place; in his route visiting France, Italy, Greece, Syria, and the isles of the Mediterranean. After a year spent at Algiers, studying and practising the African languages, he commenced his travels in that country, visiting many of the interior parts, particularly Egypt and Abyssinia; where after numerous dangers and distresses he reached the sources of the Nile, the chief object of all his labours, where he arrived in November 1770; whence returning by Nubia, and encountering astonishing difficulties, he at length, after 4 years absence arrived at Cairo the beginning of the year 1773. Whence returning through several parts of Europe, where he met with the favourable reception due to so extraordinary a traveller, he arrived in his native country after an absence of 12 years. In the course of his travels, Mr. B. met with such uncommon events, that many persons were disposed to question his veracity in the accounts he gave of them; which for many years prevented the publication of his travels, which only took place in 1790, in 5 volumes in 4to. It has been said that the king purchased Mr. Bruce's drawings for 2000l. and also defrayed the expence of engraving the plates for his great work.

the Troglodytia; not to be confounded with another nation of Troglodytes, very different in all respects, living in the forests between Abyssinia and Nubia. The myrrh of the Troglodytes was always, as now, preferred to that of Arabia. That part of Abyssinia being half overrun and settled, half wasted and abandoned, by a barbarous nation from the southward, very little correspondence or commerce has been since carried on between the Arabians and that coast; unless by some desperate adventures of Mahometan merchants, made under accidental circumstances, which have sometimes succeeded, and very often likewise have miscarried.

The most frequent way by which this Troglodyte myrrh is exported, is from Massowa, a small Abyssinian island, on the coast of the Red Sea. Yet the quantity of Abyssinian myrrh is so very small, in comparison of that of Arabia sent to Grand Cairo, that we may safely attribute to this only the reason why our myrrh is not so good in quality as the myrrh of the ancients, which was Abyssinian. Though those barbarians make use of the gum, leaves, and bark, of this tree, in diseases to which they are subject; yet as very little is wanted for such purposes, and the tree is the common timber of the country, this does not hinder them from cutting it down every day, to burn for the common uses of life; and as they never plant, or replace the trees destroyed, it is probable, that in some years the true Troglodyte myrrh will not exist; and the erroneous descriptions of the Greek physicians will lead posterity, as they have done us now, into various conjectures, all of them false, on the question, what that myrrh of the ancients was?

Though the myrrh of the Troglodytes was superior to any Arabian, yet the Greeks perceived that it was not all of equal goodness. Pliny and Theophrastus makes this difference to arise from the trees being partly wild, partly cultivated. But this is an imaginary reason; all the trees were wild. But it was the age of the tree and its health, the manner of making the cut or wound in it, the time of gathering the myrrh, and the circumstances of the climate when it was gathered, that constantly determined, and does yet determine, the quality of the drug. In order to have myrrh of the first, or most perfect sort, the savages chuse a young, vigorous tree, whose bark is without moss, or any parasite plant; and, above the first large branches, give the tree a deep wound with an axe. The myrrh which flows the first year, through this wound, is myrrh of the first growth; and never in very great quantity. This operation is performed some time after the rains have ceased; that is, from April to June; and the myrrh is produced in July and August. The sap once accustomed to issue through this gash, continues to do so spontaneously, at the return of every season: but the tropical rains, which are very violent, and continue 6 months, wash so much dirt, and lodge so much water in the cut, that in the 2d year,

the tree has begun to rot and turn foul in that part, and the myrrh is of a 2d quality, and sells in Cairo about a 3d cheaper than the first. The myrrh also produced from gashes near the roots, and in the trunks of old trees, is of the 2d growth and quality, and sometimes worse. This however is the good myrrh of the Italian shops every where but in Venice. It is of a blackish red, foul colour, solid and heavy, losing little of its weight by being long kept; and it is not easily distinguished from that of Arabia Felix. The 3d and worst kind is gathered from old wounds or gashes, formerly made, in old trees; or myrrh that, passing unnoticed, has hung upon the tree ungathered a whole year; black and earth-like in colour, and heavy, with little smell and bitterness. This apparently is the caucalis of the ancients.

Pliny speaks of stacte, as if it was fresh or liquid myrrh; and Dioscorides, (cap. 67,) says something like this also. However, it is not credible that the ancients, either Greeks or Latins, placed at such a distance, could ever see the myrrh in that state. The natives of its country say, that it hardens on the tree instantly, on being exposed to air; and I, who was several months within 4 days journey of the place where it grew, and had the savages quite at my devotion to go and come from thence, could never see the newest myrrh softer than the state it now is in; though I think it dissolved more perfectly in water, than when it had been kept. Dioscorides too mentions a kind of myrrh, which he says was green, and of the consistence of paste. But as Serapion and the Arabs say, that stacte was a preparation of myrrh dissolved in water, it is probable, that this unknown green kind of Dioscorides was, like the stacte, a composition of myrrh and some other ingredient, not a species of Abyssinian myrrh, which he could never have seen, either soft or green.

It may be remarked, that when we buy fresh or new myrrh, it has always a very strong, rancid; oily smell; and when thrown into water, globules of an oily matter swim upon the surface. This greasiness is not from the myrrh; it is owing to the savages using goats-skins anointed with butter, to make them supple, in which to put their myrrh at gathering; and in these skins it remains, and is brought to market: so that, far from its being a fault, as some ignorant druggists at Rome and Venice believe, it is a mark that the myrrh is fresh gathered, which is the best quality that myrrh of the first sort can have. Besides, far from injuring the myrrh, this oily covering must rather at first have been of service; as it certainly imprisons and confines the volatile parts of new myrrh, which escape in great quantities, to a very considerable diminution in the weight. The piece of myrrh which I send you, is what a fine tree, less than 15 inches diameter in the trunk at the bottom, wounded in 2 places, produced at one of the wounds, in the year 1771. And it may be regarded as the only unexceptionable and authentic evidence in Europe, of what the Troglodyte

myrrh was; unless it be those pieces still remaining in my collection, and a piece, somewhat smaller than yours, which I gave to the king of France's cabinet at Paris. This piece which I send you, had lost near 6 drachms Troy of its weight, between the 27th of August, 1771, and the 29th of June, 1773. It has lost a very few grains since. It was kept, as were all the other pieces, with great care in cotton, separately in a box, to prevent its losing weight by friction.

*Opocalpasum.*—At the time when I was on the borders of the Tal-Tal, or Troglodyte country, I sought to procure myself branches and bark of the myrrh tree, enough preserved to be able to draw it; but the length and ruggedness of the way, the heat of the weather, and the carelessness and want of resources of naked savages, always disappointed me. In those goat-skin bags into which I had often ordered them to put small branches, I always found the leaves mostly in powder; some few that were entire, seemed to resemble much the acacia vera, but were wider towards the extremity, and more pointed immediately at the end. In what order the leaves grew, I never could determine. The bark was absolutely like that of the acacia vera; and among the leaves I often met with a small straight weak thorn, about 2 inches long. These were all the circumstances I could combine, relative to the myrrh tree, too vague and uncertain to risk a drawing on, when there still remained so many desiderata concerning it; and as the king was obstinate not to let me go thither, after what had happened to the surgeon, mate, and boat's crew, of the Elgin Indiaman, I was obliged to abandon the drawing of the myrrh tree to some more fortunate traveller. At the same time that I was taking these pains about the myrrh, I had desired the savages to bring me all the gums they could find, with the branches and bark of the trees that produced them. They brought me, at different times, some very fine pieces of incense, and at another time, a very small quantity of a bright colourless gum, sweeter on burning than incense; but no branches of either tree, though I found this latter afterwards, in another part of Abyssinia. But at all times they brought me quantities of gum, of an even and close grain, and of a dark brown colour, which was produced by a tree called sassa: and twice I received branches of this tree in tolerable order; and of these I made a drawing. Some weeks after, walking in a Mahometan village, I saw a large tree, with the whole upper part of the trunk and the large branches so covered with great bosses and knobs of gum, as to appear monstrous: and asking further about the tree, I found that it had been brought, many years before, from the myrrh country by merchants, and planted there for the sake of its gum, with which these Mahometans stiffened the blue Surat cloths, which they got damaged from Mocha, to trade in with the Galla and Abyssinians. Neither the tree which they called sassa, nor the name, nor the gum, could allow me to doubt a

moment that it was the same as what had been brought to me from the myrrh country ; but I had the additional satisfaction to find the tree all covered over with beautiful crimson flowers, of a very extraordinary and strange construction. I began then a drawing anew, with all that satisfaction known only to those who have been conversant in such discoveries. I took pieces of the gum with me. It is very light. Galen complains that, in his time, the myrrh was often mixed with a drug which he calls opocalpasum, by a Greek name ; but what this drug was, is totally unknown to us at this day. But as the only view of the savage, in mixing another gum with his myrrh, must have been to increase the quantity ; and as the great plenty in which this gum is produced, and its colour makes it very proper for this use ; and above all, as there is no reason to think there is another gum-bearing tree, of equal qualities, in the country where the myrrh grows, it seems to me next to a proof, that this must have been the opocalpasum.

I must however confess, that Galen says the opocalpasum was so far from an innocent drug, that it was a mortal poison, and had produced very fatal effects. But as those Troglodytes, though now more ignorant than formerly, are still well acquainted with the properties of their herbs and trees, it is not possible that the savage, desiring to increase his sales, would mix them with a poison that must needs diminish them. And we may therefore without scruple suppose, that Galen was mistaken in the quality ascribed to this drug ; and that he might have imagined that people died of the opocalpasum, who perhaps really died of the physician. First, because we know of no gum or resin that is a mortal poison : 2dly, because, from the construction of its parts, gum is very ill adapted for having the activity which violent poison has ; and considering the small quantities in which myrrh is taken, and the opocalpasum could have been but in an inconsiderable proportion to the myrrh, to have killed, it must have been a very active poison : 3dly, these accidents, from a known cause, must have brought myrrh into disuse, as certainly as the Spaniards mixing arsenic with the bark, would banish that drug when we saw people die of it. Now this never was the case : it maintained its character among the Greeks and the Arabs, and so down to our days ; and a modern physician thinks it might make man immortal, if it could be rendered perfectly soluble in the human body.

Galen then was mistaken as to the poisonous quality of the opocalpasum. The Greek physicians knew little of the natural history of Arabia ; still less of that of Abyssinia ; and we who have followed them know nothing of either. This gum, being put into water, swells and turns white, and loses all its glue : it resembles gum adragant much in quality, and may be eaten safely. This specimen came from the Troglodyte country in the year 1771 : a piece of myrrh from Arabia Felix, and a piece of gum of the sassa from Abyssinia, were packed

up in another separate box, to be sent you for comparison, but forgotten by my servant. They will be sent hereafter. The sassa, the tree which produces the opocalpasum, does not grow in Arabia. Arabian myrrh is easily known from Abyssinian by the following method: take a handful of the smallest pieces, found at the bottom of the basket where the myrrh was packed, and throw them into a plate, and just cover them with water a little warm; the myrrh will remain for some time without visible alteration, for it dissolves slowly; but the gum will swell to 5 times its original size, and appear so many white spots among the myrrh. The pieces sent are, N° 1, Virgin Troglodyte myrrh. N° 2, the worst sort of Troglodyte myrrh, called cancabs. N° 3, Opocalpasum from the myrrh-country.

*XLI. An Account of a curious Giant's Causeway, or Group of Angular Columns, newly discovered in the Euganean Hills, near Padua, in Italy. By John Strange, Esq. F. R. S. Dated Venice, March 10, 1775. p. 418.*

This phenomenon is situated at Castel Nuovo, a small village near Teolo, in the Euganean hills, about 4 miles south-west of the other Giant's Causeway of Monte Rosso before described. Il Sasso di San Biasio, which is the name of the spot where this causeway is situated, is a large insulated rock, composed of the same sort of grey granite that is common to the Euganean hills, before described. The columns which form this causeway, partly against the flank of the rock, and partly round its base, are of the same substance with the rock itself, to which they adhere, as I have constantly observed in all similar groups. They are therefore of a compound nature, like the columns of Monte Rosso, and differ entirely from the common sort, which are mostly homogeneous, or of a uniform texture; as is observable in the jointed, as well as simple species of basaltes. By comparing the pieces of these columns with the fragments of the columns of Monte Rosso, before transmitted to the society, some essential difference will appear between them. Those of San Biasio, though very hard, are rather porous, of a lighter colour than the columns of Monte Rosso, and very much resemble a species of lava.

This porousness Mr. S. once before observed, and more signally too, in some basaltic columns near Achon, in the province of Auvergne, in France. The pores in the columns of both these groups are also irregularly dispersed, and of unequal size, like those of pumice stones and other common pori ignei. Those of the columns of San Biasio are commonly invested with a sort of crocus martis, frequently observed in the pores of other volcanic concretions. These properties are surely further marks in favour of the igneous origin of such columnar crystallization; especially, since they seem contrary to the principle by which the common aqueous crystals are formed, successively, et per juxta-positionem par-

tium ad partes. In fact, these crystals manifest no such porosity. The columns of Achon, though of a homogeneous substance, yet differ from the common basaltes by their immense size as well as colour, which is rather brown than black. The columns of San Biasio are likewise very large, measuring often 2 feet in diameter. They are also of the simple species, that is not jointed, and mostly quadrangular, which figure seems rather a principal characteristic of this group, being rarely observed in others. So true it is, as I formerly remarked, that some particular characteristic ever distinguishes the different groups of basaltes; which therefore cannot be too narrowly observed, before we pretend to form any opinion about their origin. Some few, but very few, chiefly of the smaller columns of San Biasio, are of a pentagonal form. But there are no hexagonal columns, which, in other basaltic groups, are the most common. The natural position of these columns, whether facing the rock, or about the bottom of it, is mostly perpendicular.

Another adjacent portion of this rock is also characterized by angular, and as it were winding strata, somewhat resembling the bending pillars of Staffa. The rock itself is also composed of angular masses, as are indeed most granites; and these masses are also ranged perpendicularly. Several emerge, as it were, from the tops and sides of the neighbouring rocks and hills, like so many stately and artificial pillars. The winding strata before-mentioned are also parallel with each other, as frequently observed in other granites, as well as common vulcanic strata in general, particularly of the harder sort. Desmarest calls the latter *Basaltes en tables*; which is a kind of vulcanic slate, formed in parallel strata of different thickness, from 2 or 3 to 5 and 6 inches. This is very common in the provinces of Velay and Auvergne, in France, where it is also used for coverings of houses. The same sort of slate is likewise common to the mountains of Genoa, many of which seem to be of vulcanic origin. These slaty tables, or parallel strata, of granite, are observed near the top of the famous San Gothard, in the ascent of that mountain on the side towards Switzerland. These strata are also ranged perpendicularly, like the other common ones in granites, and resemble Desmarest's *basaltes en tables*; affording thus another proof of the analogy remarkable between the organization of the different masses in granites, and that of common vulcanic strata in general. The former, as well as the latter, have their prismatic columns, their *basaltes en tables*, as Desmarest calls them, and *en boules*. Surely therefore these are strong proofs in favour of the common origin of both. The rocks of San Biasio abound with ferruginous vitrifications, which are frequently observable in granites; and the neighbouring tracts with lava or *pori ignei*.

*XLII. Observations on the Difference between the Duration of Human Life in*

*Towns and in Country Parishes and Villages. By the Rev. Rich. Price, D. D., F. R. S. p. 424.*

This society has lately been much obliged to Dr. Perceval, for the accounts he has communicated of the state of population at Manchester and its adjacent places. These accounts contain some facts which appear curious and important. From the last in particular, there appears to be reason for concluding, that whereas a 28th part of the inhabitants die annually in the town of Manchester, not more than a 56th part die annually in the adjacent country. This implies a difference so great between the rates of human mortality in these different situations, that some persons have thought it incredible. Dr. P. therefore offers the following observations on this subject.

In the first place, the evidence in this instance is such as seems to leave little room for doubt. From an accurate survey it appears, that the number of inhabitants in the town was 27246, in the year 1773. The number of deaths the same year (and also the average for 1772, 1773, and 1774,) was 973; that is, a 28th part of the number of inhabitants. From an equally careful survey it appears, that the number of inhabitants in that part of the parish of Manchester which lies in the country, was 13786. The number of deaths in 1772 was 246; that is, a 56th part of the number of inhabitants. The chief objection to this evidence is, that the number of deaths in that part of the parish which lies in the country is given only for one year; whereas the average of several years ought to be given. But first, the number of deaths in 1772, in the town was nearly the same with the medium for 7 years; and hence there arises a probability, that in the adjacent country, the number of deaths, in the same year, could not have been much lower than the medium. Secondly, supposing it lower, there is the highest probability, that it was not more than a 4th or 5th lower. Suppose then the true annual medium to be 300, instead of 246, and it will follow, that whereas a 28th part of the inhabitants die in the town annually, a 46th part die in the country; and this is a difference very considerable. But the difference which this survey gives between the rate of mortality in the town of Manchester and the adjacent country, is confirmed by a variety of other accounts. It may be stated in general, that whereas in great towns, the proportion of inhabitants dying annually is from 1 in 19 to 1 in 22 or 23, and in moderate towns from 1 in 24 to 1 in 28; in country parishes and villages, on the contrary, this proportion seldom exceeds 1 in 40 or 50. The proofs of this are numerous and unexceptionable. Thus, the number of inhabitants at Stockholm in 1763 was 72079. The average of deaths for the 6 preceding years had been 3802. Therefore 1 in 19 died there annually. At Rome, an account is taken every year of the number of inhabitants; and in the year 1771 it was 159675. The average of deaths for 10 years had been 7367:



therefore 1 in  $21\frac{1}{4}$  died annually. In London, at least 1 in  $20\frac{1}{4}$  of the inhabitants die annually. And, from a particular survey and a very accurate register of mortality at Northampton, it appears that 1 in  $26\frac{1}{4}$  die there annually.

Let these facts be compared with the following. In 1767, a survey was made of the inhabitants of the island of Madeira, under the direction of Dr. Thomas Heberden, and their number was found to be 64614. The average of burials for 8 preceding years had been 1293. Only 1 in 50 therefore of the inhabitants died annually.

The district of Vaud, in Switzerland, in 1766, contained 112951 inhabitants. The average of deaths for 10 preceding years had been 2504. Only 1 in 45 therefore died annually. The number of inhabitants in the parish of Ackworth, in the county of York, in 1757, was 603; and the average of deaths for 10 years had been  $10\frac{7}{8}$ , or a 56th part. In 1767, the inhabitants were increased to 728; and the annual average of deaths was  $15\frac{2}{3}$ , or nearly a 47th part.

The reason of this striking difference between the rate of human mortality in towns and in country parishes and villages must be, first, the luxury and the irregular modes of life which prevail in towns; and, secondly, the foulness of the air. But it has been inquired, whether the migrations of people from the country to towns may not produce this difference, by lessening the proportion of inhabitants that die in the country, and increasing the same proportion in towns? In answer to this inquiry, Dr. P. observes, 1st, that this difference, being a difference of near a half, it is apparently much greater than can be accounted for by any such cause. But 2dly, it should be considered, that if migrations lessen the number of deaths, they also lessen the number of inhabitants; and that it depends entirely on the ages at which the inhabitants remove from a place, whether the effect of their removal shall be lowering or raising the proportion of the annual deaths to the number of inhabitants. In the present case, the truth appears to be, that the most common age of migration from the country, is such as raises this proportion in the country. This will be evident from the following considerations. The period of life in which persons remove from the country to settle in towns, is chiefly the beginning of mature life, or from the age of 10 or 15 to 25 or 30. In infancy none migrate; and in the decline of life, it is more usual to retire from towns than to remove to them. Towns therefore will be inhabited more by people in the firmest parts of life; and, on the other hand, the country will be inhabited more by people in the weakest parts of life; and the consequence of this is, that in the country, the inhabitants must die faster in proportion to their number than they otherwise would, and that in towns they must die more slowly. In particular, the number of children is always much greater in the country than in towns; and this is a circumstance which must be extremely unfavourable to the former: for it is well known, that

there are no years of life, in which so many of a given number die, as the first 3 or 4 years. Till the age of 5, human life, like a fire beginning to burn, is very feeble; and in some situations more than  $\frac{1}{4}$ , and in others, a 3d or 4th of all that are born die before that age. After this, life grows less and less precarious, till it acquires its utmost vigour at 10 or 15; and of the living at this age, not above 1 in 70 or 80 dies annually in the worst situations; and in the best situations, not above 1 in 150 or 160. After 15, life declines, and continues to do so more and more, till it becomes quite extinct in old age. If therefore, in any situation, the inhabitants consist more of persons in mature life, and yet die faster, it must be owing to some particular causes of mortality that operate there. This is the case in all towns where any observations have been made. Manchester, in particular, is not only kept up, but increases fast, by removals to it of persons in the prime of life. The country round it increases likewise; but it is by an excess of the births above the deaths; that is, by accessions to it of children in the very feeblest part of life. This ought to raise the proportion of annual deaths to inhabitants in the country, much above the same proportion in the town; but, instead of this, it is near one half lower.

In order however to put this matter out of all doubt, it appears in fact, from the accounts furnished by Dr. Percival, that the number of inhabitants in the periods of life when mankind die fastest, that is, in the first and last stages of life, is considerably less in the town of Manchester than in the adjacent country. The number of inhabitants in the town, under 15 and above 50, is 13467; in the country, 7305. And the whole number is, in the town, 27246; in the country, 13786. In the town therefore the inhabitants, in the first and last stages of life, do not make half the whole number; but in the country, they make considerably more than half. At Ackworth likewise, in Yorkshire, the inhabitants under 15 and above 50 are more than half the whole number; and the same is true at Hale near Altringham, at Horwich, at Darwen near Blackburn in Lancashire, and at Cockey Moor near Bolton, in the same county, and yet in some of these places it appears, that not a 60th part of the inhabitants die annually. At Stockholm, in 1763, the inhabitants under the age of 5, were only a 12th; above 70, only a 46th part of the whole number. But in all Sweden, the number under 5 was a 7th; and above 70, near the 32d part of all the inhabitants: and yet 35 die in the town to 19 in the whole kingdom. This may be easily deduced from Mr. Wargentin's tables in the Collection Academique.

To the accounts which give the proportion of inhabitants to annual deaths, so high as 50 or 60 to 1, it has been further objected, that if true, it must follow, that in such situations half the inhabitants must live to 50 or 60 years of age. But were this a right inference, there would be nothing in it incredible. For though in most cities one half die in the first 2 or 3 years after birth; yet

in many country situations, the greater part live to marry: and in the parish of Ackworth particularly, it appears with undeniable evidence from the register, that one half of all born there live to the age of 46. It appears also, with equal evidence, from M. Muret's tables in the Bern Memoirs for 1766, that in 43 parishes in the district of Vaud, one half of all born there live beyond the age of 41. In truth, did all mankind lead natural and virtuous lives, that waste of the species which happens in infancy and childhood would not take place, and few would die except in old age.

But to return to Dr. Percival's account of the town and parish of Manchester. It appears from this account, that the number of children under 15, compared with the number of inhabitants between 14 and 51, is greater in the country than in the town of Manchester, in the proportion of no less than 5 to 4. It follows therefore, that though, in consequence of a constant influx of people to the town, it is more filled than the country with inhabitants in the most vigorous periods of life; yet 1 child in 4 less is born in the town than in the country. This is a remarkable circumstance, and the reasons of it must be the two following. First, the town inhabitants, being less healthy, and dying faster, have not the same strength of constitution with the country inhabitants. 2dly, in the town a smaller proportion of the adult inhabitants marry; and they marry later than in the country. The survey fully proves this; for it appears, that though the number of inhabitants at the most common marrying ages, compared with the whole number of the living above the age of 14, is smaller in the country than the town; yet the proportion of the married to the living above 14, is very nearly the same in both situations. And there are more widows and widowers in the town than in the country in the proportion of near 16 to 11. Hence we learn clearly in what manner towns operate in checking population, and preventing the increase of mankind.

Dr. Percival informs us, that the reverend and learned Dr. Tucker has been led, by some observations he has made at Bristol, to doubt whether the common opinion is right, with respect to the disproportion between the number of male and female births; and that he therefore wishes a further inquiry may be made into this subject. This has induced Dr. P. to collect the following facts, which he thinks will abundantly settle this point.

	Born Males.	Females.	Proportion.
In London for the last 110 years, or from 1664 to 1773.....	862293 ..	817072 ..	20 to 19
Paris, for 8 years .....	79693 ..	76481 ..	25 .. 24
Leyden, for 50 years .....	46773 ..	44933 ..	26 .. 25
Vienna, for 27 years, ending 1746.....	67060 ..	64893 ..	31 .. 30
Berlin, for 40 years, ending 1761.....	71188 ..	67431 ..	20 .. 19
Kurmark of Brandenburg, for 9 years, ending 1759 .....	102425 ..	96521 ..	18 .. 17
Dukedom of Magdeburgh, for 38 years, ending 1759.....	153227 ..	145985 ..	21 .. 20
All the Prussian towns, for a course of years.....	691826 ..	659072 ..	21 .. 20
In a great number of country parishes, for a course of years.....	59067 ..	56282 ..	21 .. 20

	Born Males.	Females.	Proportion.
In the same country parishes, for another period of years.....	89530 ..	84954 ..	19 to 18
Leeds, Manchester, Coventry, &c. for a period of years .....	108784 ..	103449 ..	20.. 19
In the same towns, for another period.....	57084 ..	54128 ..	20.. 19
Total.....	2388950 ..	2271201 ..	20.. 19
Sweden, for 9 years, ending 1763 .....	416007 ..	396124 ..	20.. 19

Mr. Derham, in his *Physico-Theology*, p. 175, has stated the proportion of male to female births at 14 to 13, and this proportion has ever since been generally received as the true one; but it appears from this table, that it ought to have been stated at 20 to 19. But though it appears, that the number of males born is in this proportion greater than the number of females born, yet in most places the number of males living, has been found to be less than the number of females. The reason is doubtless, that males are more short-lived than females; and this is owing partly to the peculiar hazards to which males are subject, and their more irregular modes of life; but it is owing principally to some particular delicacy in the male constitution, which renders it less durable: for there are many observations which prove, that the greater mortality of males takes place chiefly in the first and last stages of life. A few facts of this kind Dr. P. mentions, as he has just met with them.

In the parish of St. Sulpice, at Paris, during 30 years, 5 males under a year old died to 4 females. But under 10, only 13 males died to 12 females. In Stockholm, during 9 years ending in 1763, the number of still-borns amounted to 666; of whom 390 were males, and 276 females; that is, 10 to 7. The number of the living in the town above the age of 80 was, in 1760, 332; of whom 248 were females, and 84 males, or near 3 to 1. In the whole kingdom of Sweden, including all town and country inhabitants, the number of still-borns, during the 9 years just mentioned, was 19845; of whom 11424 were males, and 8421 females, or near 4 to 3. The number of the living in the whole kingdom consisted of more females than males, in the proportion of 10 to 9. It consisted of more females turned of 80 than males, in the proportion of 33 to 19; and of more females turned of 90 than males in the proportion of near 2 to 1. It appears also, that by the excess of the births above the deaths, Sweden gains every year an addition of above 20,000 inhabitants; and that in 6 years they increased from 2323195 to 2446394.

The following tables have been selected from several more of the same kind in M. Wargentin's *Memoir* on the state of population in Sweden. They are inserted here, because they fully verify most of the observations in the preceding paper, and contain more distinct and authentic information on the subject of human mortality than has ever before been met with.

TABLE I.

*Showing the order of human mortality in Sweden.*

TABLE II.

*Showing the order of human mortality at Stockholm.*

Annual deaths, being the average of 3 years 1761, 1762, & 1763.			Number of living in 1763.			Annual deaths, being the average of 3 years, 1761, 1762, & 1763.			Number of living in 1763.						
		Males.	Females.			Males.	Females.			Males.	Females.			Males.	Females.
Still-born	1324	988	Born	47216	44895	Still born	54	43	Born	1406	1340				
Died under 1	11172	9850	Living under 1	36094	35453	Died under 1	567	489	Living under 1	684	733				
Died between 1 and 3	4393	4336	Living between 1 and 3	66059	67234	Died between 1 and 3	161	170	Living between 1 and 3	1173	1348				
3.... 5	2206	2249	3.... 5	66454	67711	3.... 5	89	79	3.... 5	1022	1106				
5.... 10	2151	2057	5.... 10	130019	130758	5.... 10	71	72	5.... 10	2630	2774				
10.... 15	933	834	10.... 15	126696	128021	10.... 15	49	24	10.... 15	3151	2918				
15.... 20	711	658	15.... 20	108312	109985	15.... 20	53	30	15.... 20	3018	2865				
20.... 25	834	756	20.... 25	92299	105115	20.... 25	91	64	20.... 25	3070	4056				
25.... 30	883	863	25.... 30	88056	101003	25.... 30	121	78	25.... 30	3380	4251				
30.... 35	1020	1146	30.... 35	85936	95811	30.... 35	141	102	30.... 35	3705	4234				
35.... 40	955	923	35.... 40	74826	81453	35.... 40	118	96	35.... 40	3019	3288				
40.... 45	1180	1170	40.... 45	67448	74854	40.... 45	140	115	40.... 45	2846	3130				
45.... 50	1099	938	45.... 50	52398	59551	45.... 50	101	84	45.... 50	1775	1984				
50.... 55	1280	1113	50.... 55	47298	56646	50.... 55	105	91	50.... 55	1581	2129				
55.... 60	1177	1097	55.... 60	37086	45537	55.... 60	61	54	55.... 60	853	1329				
60.... 65	1586	1721	60.... 65	34892	44925	60.... 65	79	88	60.... 65	826	1383				
65.... 70	1237	1566	65.... 70	20649	28964	65.... 70	41	54	65.... 70	370	778				
70.... 75	1324	2041	70.... 75	15454	23159	70.... 75	33	77	70.... 75	260	574				
75.... 80	1092	1695	75.... 80	8858	13556	75.... 80	28	59	75.... 80	128	324				
80.... 85	917	1446	80.... 85	4620	7487	80.... 85	18	45	80.... 85	58	127				
85.... 90	414	650	85.... 90	1508	2694	85.... 90	7	20	85.... 90	16	51				
Above 90	215	379	Above 90	527	988	Above 90	3	11	Above 90	10	22				
Total of annual deaths.	36777	37488	Total of living at all ages.	1165489	1280905	Total of annual deaths.	2068	1902	Total of living at all ages.	33575	39404				

In this table it is observable, that the number of the living, in every equal division of life from birth, decreases continually till all become extinct; and that though the males born are more than the females born, in the proportion of 20 to 19; yet the males living of all ages are less in number, in the proportion of 1165489 to 1280905, or nearly of 10 to 11; notwithstanding which, the males that die annually are to the females as 52 to 53.

In this table it may be observed, that the number living at every age from birth, decreases only till 5. Between 5 and 10, Stockholm begins to receive recruits from the country, and they come in faster and faster till 35; after which age it appears, that more die than come in; and that the living in every subsequent period goes on decreasing continually till the end of life. It is further observable, that this table exhibits a greater difference than the former, between the mortality of males and females.

A comparison of these tables will show a striking contrast in other respects between the state of human mortality in the whole kingdom of Sweden and in its capital. In order to make this more obvious and unexceptionable, I will add the following table, deduced from all M. Wargentin's tables taken together.

TABLE III.

	In all Sweden for 9 years.		In Stockholm for 9 years.	
	Males.	Females.	Males.	Females.
Still-born.....	1 in 36	1 in 47	1 in 32	1 in 43 $\frac{1}{2}$
Died under 1 of all born	1 .. 4 $\frac{1}{2}$	1 .. 4 $\frac{1}{2}$	1 .. 2 $\frac{1}{2}$	1 .. 2 $\frac{3}{5}$
Died annually of the } living betw. 1 and 3 }	1 .. 17 $\frac{1}{2}$	1 .. 17 $\frac{1}{2}$	1 .. 7	1 .. 7 $\frac{1}{2}$
Between... 3.... 5	1 .. 34 $\frac{1}{2}$	1 .. 36	1 .. 13 $\frac{1}{2}$	1 .. 16
5.... 10	1 .. 71	1 .. 76	1 .. 34 $\frac{1}{2}$	1 .. 39
10.... 15	1 .. 149	1 .. 161	1 .. 79	1 .. 114
15.... 20	1 .. 149	1 .. 164	1 .. 59	1 .. 99
20.... 25	1 .. 108	1 .. 139	1 .. 44	1 .. 79
25.... 30	1 .. 98	1 .. 113	1 .. 33	1 .. 58
30.... 35	1 .. 85	1 .. 84	1 .. 31	1 .. 43
35.... 40	1 .. 78	1 .. 91	1 .. 26 $\frac{1}{2}$	1 .. 39
40.... 45	1 .. 56	1 .. 63	1 .. 23	1 .. 31
45.... 50	1 .. 49	1 .. 65	1 .. 19 $\frac{1}{2}$	1 .. 28
50.... 55	1 .. 37	1 .. 50	1 .. 16 $\frac{1}{2}$	1 .. 25 $\frac{1}{2}$
55.... 60	1 .. 31	1 .. 40	1 .. 14	1 .. 24
60.... 65	1 .. 23	1 .. 26	1 .. 11	1 .. 16
65.... 70	1 .. 17	1 .. 18 $\frac{1}{2}$	1 .. 9 $\frac{1}{2}$	1 .. 13 $\frac{1}{2}$
70.... 75	1 .. 11 $\frac{1}{2}$	1 .. 11 $\frac{1}{2}$	1 .. 7 $\frac{3}{5}$	1 .. 8
75.... 80	1 .. 8	1 .. 8 $\frac{1}{2}$	1 .. 4 $\frac{1}{2}$	1 .. 5
80.... 85	1 .. 5 $\frac{1}{2}$	1 .. 5 $\frac{1}{2}$	1 .. 3 $\frac{1}{2}$	1 .. 3 $\frac{1}{2}$
85.... 90	1 .. 3 $\frac{1}{2}$	1 .. 4	1 .. 2	1 .. 2 $\frac{1}{2}$
Above 90	1 .. 2 $\frac{1}{2}$	1 .. 2 $\frac{1}{2}$	1 .. 2 $\frac{3}{5}$	1 .. 2 $\frac{1}{2}$
Died of all living at all ages	1 .. 33 $\frac{1}{2}$	1 .. 36 $\frac{1}{2}$	1 .. 17 $\frac{3}{5}$	1 .. 21 $\frac{1}{2}$

*XLIII. Experiments on Animals and Vegetables, with respect to the Power of producing Heat. By John Hunter, F. R. S. p. 446.*

Reprinted with additions, in Mr. John Hunter's *Observations on Certain Parts of the Animal Economy*.

*XLIV. A Comparison of the Heat of London and Edinburgh. By John Roebuck,\* M. D., F. R. S., in a Letter to William Heberden, M. D., F. R. S. p. 459.*

I delivered to you some time ago, a register of the thermometer at Hawkhill,

\* Dr. John Roebuck was born at Sheffield, in Yorkshire, in the year 1718; and he died in 1794, at 76 years of age, in Scotland, where for many years he had conducted several important manufacturing concerns, of his own establishing, in iron, coal, and chemical productions. His father, being a manufacturer of Sheffield goods, had intended his son for the same occupation; but from the young man's promising genius, he was induced to give him a more liberal education and profession. After the common grammar school foundation at Sheffield, he was sent to Dr. Doddridge's academy at Northampton, where he pursued his studies with distinguished reputation. Hence Mr. R. was removed to the university of Edinburgh, where having gone through a regular course of studies and practice in physic and chemistry, he next spent some time at the university of Leyden, then in high reputation as a school of medicine. There, after the usual residence and course of trials, he obtained the degree of M. D., and returned to England about the end of the year 1743. Here Dr. R. first settled and practised as a physician at Birmingham; where he afterwards established a laboratory,

for 10 years; but as these observations were made at 8 o'clock in the morning and 4 in the afternoon, and yours at 8 o'clock in the morning and 2 in the afternoon, the corresponding years of the morning's observations only admit of a comparison. It appears by your register, that the mean heat at London for 9 years, from the end of 1763 to the end of 1772, at 8 o'clock in the morning, was  $47^{\circ}.4$ ; and the mean heat at Hawkhill, during the same period of time, was  $46^{\circ}$ . The difference of which is only  $1^{\circ}.4$ . A difference much less than might be expected from the difference of latitude, and not sufficient to account why nonpareils, golden rennets, peaches, nectarines, and many kinds of grapes, generally come to maturity near London, and scarcely ever near Edinburgh, without the aid of artificial heat. Before proceeding further to perplex myself with this difficulty, I procured from Hawkhill and from yourself the register of the thermometer for 3 years, at the same periods of time. And by these it appears, that the mean heat of London of these 3 years exceeded that of Edinburgh, by  $4^{\circ}.5$ . And the mean heat of the 3 hottest months in London exceeded the mean heat of the same 3 at Edinburgh, by  $5^{\circ}.8$ . And the mean heat of these 3 summer months, at 2 o'clock in the afternoon in London, exceeded the mean heat of the same months, at the same hour, in Edinburgh, by  $7^{\circ}.3$ ; which sufficiently accounts why some fruit may come to maturity in one country and not in the other: and also why corn and grass, which vegetate

for the manufacture of certain useful preparations in chemistry. Extending his practice and projects in this line, he next established a manufactory of oil of vitriol at Prestonpans, in Scotland, in the year 1749; after which he made that country his chief residence. Dr. R.'s chemical practice leading him to experiments on smelting iron stone, and preparing that metal, which he did by means of pitcoal, he was thus gradually induced to establish, at Carron, the greatest manufactory of iron in this country. Thus, by the force of his own genius and great exertions, he established three very large and profitable manufactories, the laboratory at Birmingham, the oil of vitriol works at Prestonpans, and the iron works at Carron, all which are still carried on with great emolument to the several proprietors. Unfortunately however for Dr. R. he was induced successively to relinquish each of these concerns, to employ his capital on the next in succession, and finally to that of a large concern in coal-mines, in which his whole fortune was sunk and lost; to the grievous embitterment of the latter years of his life.

From a man so deeply and so constantly engaged in the detail of active business, many literary compositions were not to be expected. It has been happily said that Dr. R. left behind him many *works*, but few *writings*. The great object he kept constantly in view, was to promote arts and manufactures, rather than to establish theories or hypotheses. The above paper, on the comparison of the heat of London and Edinburgh; with another, in these Transactions, of experiments on ignited bodies; and one in the Edinburgh Transactions, on the filling and ripening of corn, are all his essays that have been published, besides two political pamphlets. The paper on ignited bodies was occasioned by a report of some experiments made by the celebrated Buffon, from which he had inferred that matter is heavier when hot than when cold. But Dr. R.'s experiments, made with great accuracy before a committee of the R. S. at London, seem to refute that notion.— See a pretty large and circumstantial account of Dr. R.'s concerns in the Supplement to the *Encyclopædia Britannica*, from which the above particulars are extracted.

with a more temperate heat, but require longer continuance of it, may arrive at maturity in both countries. The reason why the mean heat of London exceeds that of Edinburgh, may arise principally from the difference of latitude. But the reason why the excess is greater in proportion in the 3 hottest months of the year, at the hottest time of the day, than in the winter months, arises from Edinburgh's being situated nearer to the sea than London. We might speak with more precision on this subject, if we had a register of the thermometer at Moscow, which is nearly in the same latitude as Edinburgh; though it is well known that the heat of summer is much more intense, and the cold of winter much more severe, at Moscow, than at Edinburgh. The mean heat of springs near Edinburgh seems to be  $47^{\circ}$ ; and at London  $51^{\circ}$ . It is probable, that the mean heat of good springs in any country is very nearly the mean heat of the country. A faithful account of the heat of springs in different latitudes, and of water taken from the same depth of the sea in different latitudes is yet wanted.

Mean Heat in Pall Mall, London.

Mean Heat at Hawkhill, situated about 1 mile North of Edinburgh, and 103 feet above the level of the sea.

	1772.		1773.		1774.		Mean Heat of 3 Years.			1772.		1773.		1774.		Mean Heat of 3 Years.	
	8 A. M.	2 P. M.	8 A. M.	2 P. M.	8 A. M.	2 P. M.	8 A. M.	2 P. M.		8 A. M.	2 P. M.	8 A. M.	2 P. M.	8 A. M.	2 P. M.	8 A. M.	2 P. M.
Jan.	36	38	42	44	34	39	37.3	40.3	Jan.	31.5	34.3	38.5	40.3	29.1	33	33.3	35.8
Feb.	38	42	36	41	38	44	37.3	42.3	Feb.	30.9	36.5	35.1	40.7	36.2	40.4	34	39.2
March	41	47	40	51	41	52	40.7	50	March	37	42.8	42.1	48.4	37.1	43.2	38.7	44.8
April	44	51	45	55	47	55	45.3	53.7	April	42.9	48.5	45.6	51.1	44.1	48.9	44.2	49.5
May	49	60	50	60	51	60	50	60	May	49.1	54.5	48.6	53.1	46.6	50.8	48.1	52.8
June	64	73	58	67	59	67	60.3	69	June	57.2	62.1	55.2	60.1	51.1	59.7	54.5	60.6
July	61	72	60	68	61	69	60.7	69.7	July	58.7	64.6	57.7	61.9	57.4	63.3	57.9	63.3
Aug.	60	70	62	72	62	70	61.3	70.7	Aug.	57.4	63.9	58.3	64.8	57.2	62.5	57.6	63.7
Sept.	56	65	56	63	55	63	55.7	63.7	Sept.	51.5	58.1	51.3	55.8	51.7	57.8	51.5	57.2
Oct.	56	61	51	59	48	58	51.7	59.3	Oct.	48.8	51.6	46	50.7	48.3	52.8	47.7	51.7
Nov.	45	55	40	47	40	44	41.7	48.7	Nov.	41.7	44.6	38.2	42.3	38	42	39.3	42.9
Dec.	41	44	41	45	39	43	40.3	44	Dec.	39.7	41.6	36.4	38.5	37.3	40	37.8	40
Mean	49.2	56.5	48.4	56	47.9	55.3	48.5	56	Mean	45.5	50.3	46.1	50.6	44.5	49.5	45.4	50.1

Mean Heat of 3 years morning and afternoon was 52.2.

Mean Heat of 3 years morning and afternoon was 47.7.

*XV. Experiments in a Heated Room. By Matthew Dobson, M. D. p. 463.*

*Exper.* The sweating-room of our public hospital at Liverpool, says Dr. D., which is nearly a cube of 9 feet, lighted from the top, was heated till the quicksilver stood at  $224^{\circ}$  on Fahrenheit's scale; above which the tube of the thermometer would not admit the heat to be raised. The thermometer was suspended by a string fixed to the wooden frame of the sky-light, and hung down about the centre of the room. Myself and several others were at this time inclosed in the stove, without experiencing any oppressive or painful sensation of heat, proportioned to the degree pointed out by the thermometer. Every metallic about us soon become very hot. 2. My friend Mr. Park, a surgeon, went into the stove heated to  $202^{\circ}$ . After 10 minutes, I found the pulse quickened to  $120^{\circ}$ . And to determine the increase of the animal heat, another



thermometer was handed to him, in which the quicksilver already stood at  $98^{\circ}$ ; but it rose only to  $99\frac{1}{4}$ , whether the bulb of the thermometer was inclosed in the palms of the hands, or received into the mouth.\* The natural state of this gentleman's pulse is about 65. 3. Another gentleman went through the same experiment in the same circumstances, and with the same effects. 4. One of the porters to the hospital, a healthy young man, and the pulse 75, was inclosed in the stove when the quicksilver stood at  $210^{\circ}$ ; and he remained there, with little inconvenience, for 20 minutes. The pulse, now 164, and the animal heat, determined by another thermometer as in the former experiments, was  $101\frac{1}{4}$ . 5. A young gentleman of a delicate and irritable habit, whose natural pulse is about 80, remained in the stove 10 minutes when heated to  $224^{\circ}$ . The pulse rose to 145, and the animal heat to  $102^{\circ}$ . This gentleman, who had been frequently in the stove during the course of the day, found himself feeble, and disposed to break out into sweats for 24 hours after the experiment. 6. Two small tin vessels, containing each the white of an egg, were put into the stove heated to  $224^{\circ}$ . One of them was placed on a wooden seat near the wall, and the other suspended by a string about the middle of the stove. After 10 minutes, they began to coagulate; but the coagulation sensibly quicker and firmer in that which was suspended, than in that which was placed on the wooden seat. The progress of the coagulation was as follows: it was first formed on the sides, and gradually extended itself; the whole of the bottom was next coagulated; and last of all the middle part of the top. 7. Part of the shell of an egg was peeled away, leaving only the film which surrounds the white; and part of the white being drawn out, the film sunk so as to form a little cup. This cup was filled with some of the albumen ovi, which was consequently detached as much as possible from every thing but the contact of the air and of the film which formed the cup. The lower part of the egg stood on some light tow in a common gallipot, and was placed on the wooden seat in the stove. The quicksilver in the thermometer still continued at  $224^{\circ}$ . After remaining in the stove for an hour, the lower part of the egg, which was covered with the shell, was firmly coagulated; but that which was in the little cup was fluid and transparent. At the end of another hour it was still fluid, except on the edges where it was thinnest; and here it was still transparent; a sufficient proof that it was dried, not coagulated. 8. A piece of bees wax, placed in the same situation with the albumen ovi of the preceding experiment, and exposed to the same degree of heat in the stove, began to melt in 5 minutes: another piece suspended by a string, and a 3d piece put into the tin vessel and suspended, began likewise to liquify in 5 minutes.

\* The scale of the thermometer, which was suspended by the string about the middle of the room, was of metal; this was the only one I could then procure, on which the degrees ran so high as to give any scope to the experiment. The scale of the other thermometer, which was employed for ascertaining the variations in the animal heat, was of ivory.—Orig.

*Observations.*—That heated air should have such a speedy and powerful effect in quickening the pulse, while the animal heat is little altered from its natural standard; that the human body should so easily bear to be surrounded with air heated to  $224^{\circ}$ ; that the albumen ovi, which begins to coagulate in water at  $150^{\circ}$ , should remain fluid in  $224^{\circ}$ ; and that the same albumen ovi, still placed in air heated to  $224^{\circ}$ , should coagulate if in contact either with tin or its own shell, are facts as singular as they are difficult of explanation. From the different effects of heated air on the pulse and the heat of the body, do we not discover the fallacy of that theory of animal heat which has been adopted by Boerhaave and other celebrated physiologists? They suppose that animal heat is produced by the attrition of the globules of the circulating fluids against the sides of the containing vessels; but in several of the preceding experiments, the circulation was amazingly quickened with little increase of the animal heat. But whence is it that the human body can bear without immediate injury, to be surrounded with air heated to  $224^{\circ}$ ? And whence is it, that the albumen ovi does not coagulate in this degree of heat? Is it that fire as it passes into some bodies becomes latent, agreeable to a doctrine which has for some time been taught at Edinburgh by Professor Black? Or does fire become fixed and quiescent, according to a similar system adopted by Dr. Franklin? \* Air we know exists either in a fixed or elastic state; and fire may in like manner exist in bodies, either in a latent, fixed, and quiescent; or in a sensible, fluid, and active state. Agreeable to this idea, the bees wax receives the fire in an active state, and dissolves; while the human body and the albumen ovi, receiving the fire in a latent state, are little altered in their temperature. Let each of these however be put in contact with a different body, tin for instance; and though the heat of the air continues the same, yet the fire no longer enters in a latent state, but with all its sensible and active powers; for the albumen ovi suspended in a tin vessel soon coagulates; and the human body, covered with the same metal, would quickly experience an intolerable and destructive degree of heat. Or are the above phenomena more satisfactorily explained, by considering different bodies as possessing different conducting powers; some being strong, others weak conductors of fire? All those bodies then which are weak conductors of fire from air, may be placed in air, without receiving the heat of this medium. Hence the albumen ovi remains fluid in air heated to  $224^{\circ}$ . Hence likewise the frog, the lizard, the camelion, &c. retain their natural temperature, and feel cold to the touch, though perpetually surrounded with air hotter than their own bodies. Hence also, the human body keeps nearly its own temperature, in a stove heated to

\* Exper. and Observ. p. 346 and 412.

224°; or may even pass without injury into air heated to a much greater degree, according to the observations of Duhamel and Tillett, published in the Memoirs of the Acad. of Sciences for 1761. On the other hand, all those bodies which are powerful conductors of fire from air, are influenced in proportion when surrounded with this medium. The bees wax melted from the mere contact of the air in experiment 8; and in experiment 6, the albumen ovi was coagulated on the intervention of another body, which is a strong conductor of fire from air. But whether this method of reasoning on the natural cause of these effects be just or not, the final cause is obvious, and is to be resolved into the wise and benevolent appointment of the Almighty. Man is happily so framed, as to possess a power of keeping nearly the same tenor of heat, in all the variations of the temperature of the air in summer and in winter, in hot and cold climates; and consequently changes his situation on the surface of the globe, with much less inconvenience or injury, than he could otherwise have done. The same power likewise happily adapts different animals to their respective destinations. The lizard and the camelion remain cool under the equator, while the whale and porpoise retain a degree of heat above that of the human body, though surrounded with the waters of the coldest Northern Seas, and amidst mountains of ice in the neighbourhood of the Pole.

*XLVI. Calculations in Spherical Trigonometry Abridged. By Israel Lyons.*  
p. 470.

Since astronomical observations have been made with much greater precision than formerly, it has become requisite that the calculations corresponding to them should likewise be made to much greater degrees of exactness. The ancient astronomers desired only to make their observations and computations agree within a part of a degree; succeeding ones were satisfied when they corresponded within a minute; but no less exactness than seconds will content the moderns. The rules in spherical trigonometry being reduced to operations by logarithms, it is necessary to use such a number of figures in the tables as will produce the required precision; this is very different in the various parts of the quadrant, insomuch that if the arc is only 1 degree, 4 places of decimals in the logarithm of a sine are sufficient to determine the arc to which it belongs within a second: whereas if the arc is 89°, there is a necessity of using 8 figures for the same purpose: thus, the logarithm sine of 89° 0' 0" is 9.9999338, the same 7 figures as for the logarithm sine of 89° 0' 1". From this consideration it follows, that the analogies commonly laid down and used for the solutions of spherical triangles, are not in all cases equally convenient, and I might say, equally accurate; and that it would be more easy and exact in calculations to find what was required, by means of sines of arcs, which, being small, require the use of only a few places of figures. Now the cases which often occur in

astronomy, where spherical trigonometry can only be of use, are generally of such a nature that we know nearly, or at least within a few degrees, what the required side or angle is; there is nothing therefore wanted but to find how much this quantity, or first approximation, differs from the true value of the side or angle. Thus; in calculating the right ascension of any point of the ecliptic, whose longitude and declination are known, instead of finding the right ascension immediately, it will be more convenient to seek for the difference between the longitude and right ascension immediately, and as it never exceeds  $2\frac{1}{2}^{\circ}$ , 4 or 5 places of figures will always be sufficient to determine it within a second. And in other similar cases, rules might be made agreeable to the exigency of each particular case, which would be better than the application of the general method of solution. Some examples of which shall be shown in the following paper: the design of which is to point out a method of solving several of the most useful questions in spherical trigonometry, in a manner somewhat similar to that used in approximating to the roots of algebraic equations. This method is founded on the following

*Lemma.*—If the radius be supposed equal to unity, the sine of the sum of 2 arcs,  $\alpha$  and  $\beta$ , is equal to  $\sin. \alpha + \cos. \alpha \times \sin. \beta - \sin. \alpha \times \text{vers.} \sin. \beta$ . And its cosine =  $\cos. \alpha - \sin. \alpha \times \sin. \beta - \cos. \alpha \times \text{vers.} \sin. \beta$ . For let the arc  $\alpha$  be RA, fig. 7, pl. 13, and the arc  $\beta$  be AB, their sines Aa, BD, respectively; then Bb, being drawn perpendicular to the radius CR, will be the sine of  $\alpha + \beta$ . Draw dp and An parallel to CR. Then, by similar triangles, CA : Ca :: BD : Bp, and CA : Aa :: AD : np. Therefore, Bb (= Aa + Bp - pn) = Aa +  $\frac{Ca \times BD}{CA} - \frac{Aa \times AD}{CA}$ ; that is,  $\sin. (\alpha + \beta) = \sin. \alpha + \cos. \alpha \times \sin. \beta - \sin. \alpha \times \text{vers.} \sin. \beta$ . In the same manner, drawing dq parallel to Aa, we may prove cb (= ca - bq - aq) =  $CA - \frac{Aa \times BD}{CA} - \frac{Ca \times AD}{CA}$ , or  $\cos. (\alpha + \beta) = \cos. \alpha - \sin. \alpha \times \sin. \beta - \cos. \alpha \times \text{vers.} \sin. \beta$ .

In what follows, for brevity sake, the arc is expressed by a Greek letter; its sine by the capital character; and the cosine by the small italic character of the same letter. In this notation, the 2 theorems will stand thus,  $\sin. (\alpha + \beta) = A + AB - A \times \text{vs.} \beta$ , and  $\cos. (\alpha + \beta) = a - AB - a \times \text{vs.} \beta$ .

*Corol. 1.*—Since the tangent is equal to the sine divided by the cosine, we shall have  $\text{tang.} (\alpha + \beta) = \frac{A + AB - A \times \text{vs.} \beta}{a - AB - a \times \text{vs.} \beta} = \frac{A}{a} + \frac{B}{a^2} + \frac{A}{a^3} \times \text{vs.} \beta$  nearly.

*Corol. 2.*—If we change the sign of  $\beta$ , we shall have  $\sin. (\alpha - \beta) = A - AB - A \times \text{vs.} \beta$ .  $\cos. (\alpha - \beta) = a + AB - a \times \text{vs.} \beta$ . And  $\text{tang.} (\alpha - \beta) = \frac{A}{a} - \frac{B}{a^2} + \frac{A}{a^3} \times \text{vs.} \beta$ .

By the help of these theorems, knowing nearly what any quantity in a spherical triangle is, we may find its correction, thus: if we have to find the cosine

of an arc, which arc we know is nearly equal to  $\alpha$  whose cosine is  $a$ . Suppose the arc to be  $\alpha - \beta$ , and its cosine  $a + c$ . Then  $a + c = \cos. (\alpha - \beta) = a + AB - a \times \text{vs. } \beta$ . Therefore,  $B = \frac{c}{A} + \frac{a}{A} \times \text{vers. } \beta$ . The first term  $\frac{c}{A}$  will always give a near approximation to the value of  $\sin. \beta$ ; and  $\beta$  being found, the correction,  $\frac{a}{A} \times \text{vs. } \beta$ , or  $\cot. \alpha \times \text{vs. } \beta$ , may be found and added to it. Among the tables requisite to be used with the Nautical Almanack, is table 4 for parallax, p. 19, which shows the value at sight of such quantities as  $\text{vs. } \beta \times \cot. \alpha$ , the arc  $\beta$  being found in the first column of the table, and  $\alpha$  at the top. This table I have calculated only to arcs under  $63'$ ; but it would be found useful to have a table ready computed for all arcs under  $5^\circ$ .

PROB. 1.—*If the two legs, AB and BC, of the spherical triangle ABC right-angled at B, are given, to find the hypotenuse AC, the leg BC, being small in comparison of AC.* Fig. 8, pl. 13.—Let  $AB = \alpha$ ,  $BC = \beta$ , and suppose  $AC = \alpha + \zeta$ ,  $\alpha$  being a near approximation to  $AC$ , and  $\zeta$  the small arc to be added to  $AB$  to make it equal to  $AC$ ; then  $\cos. AC = \cos. AB \times \cos. BC$ ; that is, according to our notation,  $a - AZ - (a \times \text{vs. } \zeta) = ab$ . Whence  $z = \frac{a - ab}{A} - (\frac{a}{A} \times \text{vs. } \zeta) = \cot. \alpha \times \text{vs. } \beta - \cot. \alpha \times \text{vs. } \zeta$ .

Ex.—Let  $AB$  be  $75^\circ 0'$ , and  $BC$   $20^\circ 0'$ , then the computation will be as follows:

Cotangent $AB$ .....	9.4280
Versed sine $BC$ .....	8.7804
Sum $\zeta$ nearly $55' 33''$ .....	sine 8.2084
Correction .....	- 7 from tab. 4 Nautical Almanack.
Therefore $\zeta = 55' 26''$ , and $AC = 75^\circ 55' 26''$ .	

By this problem, the distance of the sun may be found from a planet whose latitude and difference of longitude are known.

PROB. 2.—*Having the hypotenuse, AC, and one of the angles A, to find the base AB.*—Let  $AC = \beta$ ,  $BAC = \alpha$ , fig. 8, and suppose  $AB = \beta - \zeta$ , then  $\cos. A = \cot. AC \times \text{tang. } AB$ , or  $a = \frac{b}{B} \times \frac{B}{b} - \frac{z}{b^2} + \frac{B \times \text{vs. } \zeta}{b^3} = 1 - \frac{z}{Bb} + \frac{B \times \text{vs. } \zeta}{b^2}$ .

Whence  $z = Bb \times (1 - a) + \frac{B}{b} \times \text{vs. } \zeta = \frac{1}{2} 2\beta \times \text{vs. } \alpha + \text{tang. } \beta \times \text{vs. } \zeta$ .

Ex.—Let  $A = 23^\circ 28' 15''$ , and  $AC = 10^\circ 0' 0''$ .

Sine $2AC$ $20^\circ 0'$ .....	9.5340
Versed sine $A$ .....	8.9177
Sum .....	8.4517
Log. 2. ....	0.3010
Dif. $\zeta$ nearly .....	48' 39" sine 8.1507
Correction .....	+ 6
$\zeta$ .....	48 45, and $BC = 159^\circ 11' 15''$ .

By this problem, the right ascension of any point of the ecliptic, whose obliquity and longitude are known, may be found.

PROB. 3. *Supposing the same things known as in the last, to find the perpendicular BC, when the hypotenuse is nearly a quadrant.*—Let  $A = \alpha$ ,  $AC = \beta$ , fig. 8, as before, and suppose  $BC = \alpha - \zeta$ ; then  $\sin. BC = \sin. AC \times \sin. A$ , or

$A - az = A \times \text{vs. } \zeta = AB$ , whence  $z = \frac{A - BA}{a} = \frac{A}{a} \times \text{vs. } \zeta = \text{tang. } a \times \text{co. ver. sin. } \beta - t. a \times \text{vs. } \zeta$ .

Ex.—Let  $A = 23^\circ 28' 15''$ , and  $AC = 80^\circ 0'$ .

Tang. $A$ .....	9.6377
Vers. sin. co. $AC$ , $10^\circ$ .....	8.1816
Sum, $\zeta$ nearly.... $22^\circ 41'$ .....	sine 7.8193
Correction .....	-1
Gives $\zeta$ .....	$22^\circ 40'$ , and $BC = 23^\circ 5' 35''$ .

This problem will be of use to find the declination of the ecliptic, and the latitude of a planet near the limits. And these 3 instances will suffice for an application of this method to right-angled spherical triangles; we shall now give 2 problems of oblique triangles.

PROB. 4. Suppose  $ABC$  to be a spherical triangle, in which are given the two sides  $AB$ ,  $BC$ , with the included angle  $B$ , to find the third side  $AC$ . Fig. 9.

Solut. 1. Let  $ABC = \beta$ ,  $BC = a$ ,  $AB = \delta$ . Put  $AC = \beta + \zeta$ ,  $\beta$  being an approximate value of  $AC$ , when the two legs are nearly quadrants. Now the cosine of  $AC$  being equal to  $bDA + da$ ,\* we shall have  $b - bz = (b \times \text{vs.})$ ;  $\zeta = bDA + da$ : and  $z = \frac{b - bDA - da}{B} = \frac{b}{B} \times \text{vs. } \zeta$ . But  $1 - DA - da = \text{vs. } (\delta - a)$ , which put  $= w$ . Then  $z = \frac{bw}{B} + \frac{bda - da}{B} = \frac{b}{B} \times \text{vs. } \zeta = \text{cot. } \beta \times \text{vs. } (d - a) - \cos. \delta \times \cos. a \times \text{tang. } \frac{1}{2} \beta - \text{cot. } \beta \times \text{vs. } \zeta$ . Therefore  $\zeta$  is the difference of two arcs whose sines are  $\text{cot. } \beta \times \text{vs. } (\delta - a)$ , and  $\cos. \delta \times \cos. a \times \text{tang. } \frac{1}{2} \beta$ , the difference of these two arcs being diminished by the correction  $\text{cot. } \beta \times \text{vs. } \zeta$ .

Ex.—Suppose....  $B = 51^\circ 12' 5''$

$AB = 87^\circ 57' 51''$   
 $BC = 87^\circ 20' 34''$

Cotangent  $B$ ..... 9.9053  
 Vers. sine  $AB - BC$   $0^\circ 37' 17''$ ... 5.7693

Tang.  $\frac{1}{2} B$   $25^\circ 36'$ ..... 9.6804  
 Cosine  $AB$ ..... 8.5506  
 Cosine  $BC$ ..... 8.6661  
 2d arc  $2' 43''$ ..... sine 6.8971

Sum = 1st arc  $0' 10''$ .... sine 5.6746  
 The difference of these two arcs,.....  $2' 33''$   
 Subtracted from the value of the angle  $B$ ,.....  $51^\circ 12' 5''$   
 Leaves  $AC$ ,.....  $51^\circ 9' 32''$

The correction  $\text{cot. } \beta \times \text{vs. } \zeta$  in this example is 0.

This solution is very convenient to find the distance of two zodiacal stars, having their latitudes and difference of longitude.

Solut. 2. Let  $\tau$  be an arc whose cosine  $t = b \times \cos. \delta - a = bda + bDA$ , and suppose  $AC = \tau - \zeta$ , then  $t + TZ = t \times \text{vs. } \zeta = bDA + da = t - bda + da$ .

\* It is a well known theor. that  $\sin. BA \times \sin. BC : r^2 = \text{vs. } AC - \text{vs. } (AB - BC) : \text{vs. } B$ ; that is,  $\sin. BA \times \sin. BC : r^2 = \cos. (AB - BC) - \cos. AC : r - \cos. B$ . Or, in the author's notation, putting  $r = 1$ ,  $DA : 1 = \cos. (\delta - a) - \cos. AC : 1 - b$ . Therefore  $DA - bDA = \cos. (\delta - a) - \cos. AC$ . Or,  $\cos. AC = bDA - DA - \cos. (\delta - a)$ . For  $\cos. (\delta - a)$  substitute its value as expressed in the second corollary of the lemma, and there arises the author's equation,  $\cos. AC = bDA + da$ .

—Orig. S. HORSLEY.

Whence  $z = da \times \frac{1-b}{T} + \frac{t}{T} \times \text{vers. } \zeta = \text{cosec. } \tau \times \cosin. \alpha \times \cosin. \delta \times \text{vs. } \beta + \cot. \tau \times \text{vs. } \zeta$ .

This solution is useful to find the distance of the moon from a star at some distance from the ecliptic; in which case it coincides with the rule given by the Astronomer Royal, Phil. Trans. 1764, vol. 54, and which, taking in the correction here given,  $\cot. \tau \times \text{vs. } \zeta$ , will always be exact to a second. It is also of use to find the declination of a star, whose longitude and latitude and obliquity of the ecliptic are known.

*Solut. 3.* Let the angle B be small, and the two legs AB, BC, very unequal; then the side AC will be nearly  $AB - BC$ . Fig. 10. Put this =  $x$ , and suppose  $AC = x + \zeta$ , then  $\cos. AC = k - KZ - k \times \text{vs. } \zeta = ad + AD - KZ - k \times \text{vs. } \zeta = b_{AD} + ad$ , whence  $z = \frac{DA - b_{AD}}{K} - \frac{k}{K} \times \text{vs. } \zeta = \sin. \delta \times \sin. \alpha \times \text{vs. } \beta \times \text{cosec. } \delta - \alpha - \cot. k \times \text{vs. } \zeta$ .

Ex.—Let  $AB = 94^\circ 36' 58''$   
 $BC = 23 \ 28 \ 24$   
 $B = 24 \ 54 \ 24$  } as in the example to sol. 2.

$AB - BC = 71 \ 8 \ 34$	cosecant 0.02396
Sine AB .....	9.99859
Sine BC .....	9.60023
Versed of B .....	8.96851
Sum = $\zeta$ nearly $2^\circ 14' 11''$ .....	sine 8.59129

The value of  $\zeta$  being without the limits of tab. 4, in the tables requisite to be used with the Nautical Almanack, the correction  $\cot. x \times \text{vs. } \zeta$  must be computed thus:

Cot.  $x$  . . . . . 9.533

V. sin.  $\zeta$  . . . . . 6.881

Sum = cor.  $0' 53''$ , sine 6.414, this subtracted from the first value of  $\zeta$ , leaves  $\zeta = 2^\circ 13' 18''$ , which added to  $\delta - \alpha$ , gives the side  $AC = 73^\circ 21' 52''$ . This solution will help to find the sun's altitude near noon.

I have dwelt the longer on this problem because it is one that is very commonly required in astronomical calculations, and the operation by the rules of spherical trigonometry, in this as well as the next, is rather troublesome.

PROB. 5.—*Supposing the same things given, to find either of the angles, as for instance c opposite the side AB. Fig. 10.*—We have  $\cot. c = \cot. B \times \cos. BC - \sin. BC \times \cot. AB \times \text{cosec. } B = \frac{ba}{B} - \frac{Ad}{BD}$ . Let  $\mu$  be an angle whose  $\cot. \frac{m}{M} = \cot. \beta \times \sin. \delta - \alpha \times \text{cosec. } \delta = \frac{baD - bAd}{BD}$ , and suppose  $c = \mu + \zeta$ ; then  $\cot. c = \frac{m}{M} - \frac{z}{M^2} + \frac{m \cdot \text{vs. } \zeta}{M^2} = \frac{baD - Ad}{BD}$ . Whence  $z = M \times \frac{Ad - bAd}{BD} + \frac{m}{M} \times \text{vs. } \zeta = \sin.^2 \mu \times \sin. \alpha \times \cot. \delta \times \text{tang. } \frac{1}{2} \beta + \cot. \mu \times \text{vs. } \zeta$ .

Ex.—Let.....	AB = 94° 36' 58"		
	BC = 23 28 54		
	B = 24 54 24.....	Cotang.	0.3331770
Dif. AB and BC = 71 8 34.....		Sine	9.9760412
		Cosecant AB	0.0014080
	$\mu = 26 3 44$ .....	Cot.	10.3166262
Sin. $\mu$ .....	9.286		
Sin. BE.....	9.600		
Cot. AB.....	8.909		
Tang. $\frac{1}{2}B$ .....	9.344		

Sum =  $\zeta = 4' 44''$  sine 7.139, this subtracted from  $\mu$ , leaves the angle  $c = 25^\circ 59' 0''$ .

This problem will be of use to find the right ascension of a star whose longitude and latitude, and obliquity of the ecliptic are known, or to find the sun's azimuth at any hour in a given latitude.

*XLVII. Further Experiments and Observations in a Heated Room. By Charles Blagden, M. D., F. R. S. p. 484.*

On the 3d of April, (says Dr. B.) nearly the same party as before,\* together with Lord Seaforth, Sir George Home, Mr. Dundas, and Dr. Nooth, went to the heated room in which the experiments of the 23d of Jan. were made. Dr. Fordyce had ordered the fire to be lighted the preceding day, and kept up all night; so that every thing contained in the room, and the walls themselves, being already well warmed, we were able to push the heat to a much higher degree than before. In the course of the day several different sets of experiments were going on together; but to avoid confusion, it will be necessary to relate each series by itself, without regard to the order of time; beginning with that series which serves as a continuation of our former experiments.

Soon after our arrival, a thermometer in the room rose above the boiling point; this heat we all bore perfectly well, and without any sensible alteration in the temperature of our bodies. Many repeated trials, in successively higher degrees of heat, gave still more remarkable proofs of our resisting power. The last of these experiments was made about 8 o'clock in the evening, when the heat was at the greatest: a very large thermometer, placed at a distance from the door of the room, but nearer to the wall than to the cockle, and defended from the immediate action of the cockle by a piece of paper hung before it, rose 1 or 2 degrees above  $260^\circ$ : another thermometer, which had been suspended very near the door, stood some degrees above  $240^\circ$ . At this time I went into the room, with the addition to my common clothes, of a pair of thick worsted stockings drawn over my shoes, and reaching some way above my knees; I also put on a pair of gloves, and held a cloth constantly between my face and the cockle: all these precautions were necessary to guard against the scorching of the red-hot iron. I remained 8 minutes in this situation, frequently walking

\* See the former experiments, p. 604, of this vol. of these Abridgements.



about to all the different parts of the room, but standing still most of the time in the coolest spot, near the lowest thermometer. The air felt very hot, but still by no means to such a degree as to give pain: on the contrary, I had no doubt of being able to support a much greater heat; and all the gentlemen present, who went into the room, were of the same opinion. I sweated, but not very profusely. For 7 minutes my breathing continued perfectly good; but after that time I began to feel an oppression in my lungs, attended with a sense of anxiety; which gradually increasing for the space of a minute, I thought it most prudent to put an end to the experiment, and immediately left the room. My pulse, counted as soon as I came into the cool air, for the uneasy feeling rendered me incapable of examining it in the room, was found to beat at the rate of 144 pulsations in a minute, which is more than double its ordinary quickness. To this circumstance the oppression on my breath must be partly imputed, the blood being forced into my lungs quicker than it could pass through them; and hence it may very reasonably be conjectured, that should a heat of this kind ever be pushed so far as to prove fatal, it will be found to have killed by an accumulation of blood in the lungs, or some other immediate effect of an accelerated circulation;\* for all the experiments show, that heating the air does not make it unfit for respiration, communicating to it no noxious quality except a power of irritating. In the course of this experiment, and others of the same kind by several of the gentlemen present, some circumstances occurred to us which had not been remarked before. The heat, as might have been expected, felt most intense when we were in motion; and, on the same principle a blast of the heated air from a pair of bellows was scarcely to be borne; the sensation in both these cases exactly resembled that felt in our nostrils on inspiration. The reason is obvious; when the same air remained for any time in contact with our bodies, part of its heat was destroyed, and consequently we came to be surrounded with a cooler medium than the common air of the room; whereas when fresh portions of the air were applied to our bodies in such a quick succession, that no part of it could remain in contact a sufficient time to be cooled, we necessarily felt the full heat communicated by the stove. It was observed that our breath did not feel cool to the fingers unless they were held very near the mouth; at a distance the cooling power of the breath did not sufficiently compensate the effect of putting the air in motion, especially when we breathed with force.

A chief object of this day's experiments was, to ascertain the real effect of

\* Since this experiment, I have observed the mucus from my lungs to be more serous than before, and to incline more to a saltish taste, though the lungs themselves seem perfectly sound in all other respects; which raises a suspicion that some of the smaller arteries suffered a degree of dilatation from the increased impulse of the blood.—Orig.

our clothes in enabling us to bear such high degrees of heat. With this view I took off my coat, waistcoat, and shirt, and in that situation went into the room, as soon as the thermometer had risen above the boiling point, with the precaution of holding a piece of cloth constantly between my body and the cockle, as the scorching was otherwise intolerable. The first impression of the heated air on my naked body was much more disagreeable than I had ever felt it through my clothes; but in 5 or 6 minutes a profuse sweat broke out, which gave me instant relief, and took off all the extraordinary uneasiness: at the end of 12 minutes, when the thermometer had risen almost to  $220^{\circ}$ , I left the room, very much fatigued, but no otherwise disordered; my pulse made 136 beats in a minute. On this occasion I felt nothing of that oppression on my breath, which became so material a symptom in the experiment with my clothes when the thermometer had risen to  $260^{\circ}$ : this may be partly explained by the less quickness of my pulse, the difference being at least 8 beats in a minute, and probably more, as in the experiment without my shirt the pulsations were counted before I had left the room; but there is a further circumstance to be taken into consideration, that the experiment attended with oppression on the breath was made in the evening after a very plentiful meal, whereas the other was made in the forenoon, some hours after a moderate breakfast. The unusual degree of fatigue which I felt from the experiment without my shirt, must be ascribed in great measure to the more violent effort which the living powers were obliged to exert, in order to preserve the due human temperature, when such hot air came into immediate contact with my body. In the present case it appears beyond all doubt, that the living powers were very much assisted by the perspiration, that cooling evaporation which is a further provision of nature for enabling animals to support great heats. Had we been provided with a proper balance, it would undoubtedly have rendered the experiment more complete, to have taken the exact weight of my body at going into, and coming out of, the room; as, from the quantity lost, some estimate might be formed of the share which the perspiration had in keeping the body cool; probably its effect was very considerable, but by no means sufficient to account for the whole of the cooling, and certainly not equable enough to keep the temperature of the body to such an exact pitch: for it should here be remarked, that during all the experiments made this day, whenever I tried the heat of my body, the thermometer always came very nearly to the same point; I could not perceive even the small difference of 1 degree, which was observed in our former experiments. Should these considerations however be thought insufficient to prove that evaporation was not the sole agent in keeping the body cool, I believe that Dr. Fordyce's experiments in moist air will be found to remove all doubts on this subject. Several of the gentlemen present, as well as myself, went into the room without shirts many

times afterwards, when the thermometer had risen much higher, almost to  $260^{\circ}$ , and found that we could bear the heat very well, though the first sensation was always more disagreeable than with our clothes. In all the experiments made this day it was observed, that the thermometer did not sink so much in consequence of our stay in the room as on the 23d of Jan. ; probably because a much larger mass of matter had been heated by the longer continuance of the fire.

Our own observations, with those of M. Tillet, in the Mem. of the Acad. of Sciences, for 1764, had given us good reason to suspect, that there must have been some fallacy in the experiment with a dog, made at the desire of Dr. Boerhaave, and related in his Elements of Chemistry. To determine this matter more exactly, we subjected a bitch weighing 32 lb; to the following experiment. When the thermometer had risen to  $220^{\circ}$ , the animal was shut up in the heated room, inclosed in a basket, that its feet might be defended from the scorching of the floor, and with a piece of paper before its head and breast to intercept the direct heat of the cockle. In about 10 minutes it began to pant and hold out its tongue, which symptoms continued till the end of the experiment, without ever becoming more violent than they are usually observed in dogs after exercise in hot weather ; and the animal was so little affected during the whole time, as to show signs of pleasure whenever we approached the basket. After the experiment had continued half an hour, when the thermometer had risen to  $236^{\circ}$ , we opened the basket, and found the bottom of it very wet with saliva, but could perceive no particular fœtor. We then applied a thermometer between the thigh and flank of the animal ; in about a minute the quicksilver sunk down to  $110^{\circ}$  : but the real heat of the body was certainly less than this, for we could neither keep the ball of the thermometer a sufficient time in proper contact, nor prevent the hair, which felt sensibly hotter than the bare skin, from touching every part of the instrument. I have since found, that the thermometer held in the same place, when the animal is perfectly cool and at rest, will not rise above  $101^{\circ}$ . At the end of 32 minutes the bitch was permitted to go out of the room ; on coming into the cold air she appeared perfectly brisk and lively, not in the least injured by the heat, and has now continued very well above a month. Our experiment therefore differs, in every essential circumstance of the event, from that related by Dr. Boerhaave. With respect to this last it is remarkable, if the facts be properly represented, that an intolerable stench arose from the dog ? and that an assistant dropped down senseless on going into the stove.

To prove that there was no fallacy in the degree of heat shown by the thermometer, but that the air which we breathed was capable of producing all the well-known effects of such a heat on inanimate matter, we put some eggs and a beef-steak on a tin frame, placed near the standard thermometer, and farther.

distant from the cockle than from the wall of the room. In about 20 minutes the eggs were taken out, roasted quite hard; and in 47 minutes the steak was not only dressed, but almost dry. Another beef-steak was rather over-done in 33 minutes. In the evening, when the heat was still greater, we laid a 3d beef-steak in the same place: and as it had now been observed, that the effect of the heated air was much increased by putting it in motion, we blew upon the steak with a pair of bellows, which produced a visible change on its surface, and seemed to hasten the dressing; the greatest part of it was found pretty well done in thirteen minutes.

About the middle of the day 2 similar earthen vessels, 1 containing pure water, and the other an equal quantity of the same water with a bit of wax, were put upon a piece of wood in the heated room. In  $1\frac{1}{4}$  hour the pure water was heated to  $140^{\circ}$  of the thermometer, while that with the wax had acquired a heat of  $152^{\circ}$ , part of the wax having melted and formed a film on the surface of the water, which prevented the evaporation. The pure water never came near the boiling point, but continued stationary above an hour at a much lower degree; a small quantity of oil was then dropped into it, as had before been done to that with the wax; in consequence of which, the water in both the vessels came at length to boil very briskly. A saturated solution of salt in water, put into the room, was found to heat more quickly, and to a higher degree, than pure water, probably because it evaporated less; but it could not be brought to boil till oil was added, by means of which it came towards evening into brisk ebullition, and consequently had acquired a heat of  $230^{\circ}$ . Some rectified spirit of wine in a bottle slightly corked, which had been immersed into this solution of salt while cold, began to boil in about 2 hours, and soon afterwards was totally evaporated. Perhaps no experiments hitherto made, furnish more remarkable instances of the cooling effect of evaporation, than these last facts; a power which appears to be much greater than has commonly been suspected. The evaporation itself, however, was more considerable in our experiments than it can be in almost any other situation, because the air applied to the evaporating surface was uncommonly hot, and at the same time not more charged with moisture than in its ordinary state. A powerful assistant evaporation must undoubtedly prove, in keeping the living body properly cool, when exposed to great heats; but it can act only in a gross way, and by no means in such a nice proportion to the momentary exigencies of the animal, as would be requisite for the exact preservation of its temperature: that other provision of nature which seems more immediately connected with the powers of life, is probably the great agent in preserving the just balance of temperature; exerting a greater effort in proportion as the evaporation is deficient, and a less effort as the evaporation increases. This idea corresponds with the general analogy of the animal economy, the nicer balances of which

are almost universally effected in that part of the body which is formed with the most subtle organization.

The heated room will, I hope, in time become a very useful instrument in the hands of the physician. Hitherto the necessary experiments have not been made to direct its application with a sufficient degree of certainty. However, we can already perceive a foundation for some distinctions in the use of this uncommon remedy. Should the object in view be to produce a profuse perspiration, a dry heat acting on the naked body would most effectually answer that purpose. The histories of dropsies and some other diseases, supposed to have been cured by such means, are well known to every physician. In some cases also, a moist heat, and in others heat transmitted through a quantity of clothes, might have their peculiar advantages. That the danger likely to ensue from such applications is less than has been commonly apprehended, our former experiments gave sufficient reason to believe; and the same was amply confirmed by those which make the subject of this paper. For during the whole day, we passed out of the heated room, after every experiment, immediately into the cold air, without any precaution; after exposing our naked bodies to the heat, and sweating most violently, we instantly went out into a cold room, and staid there even some minutes before we began to dress; yet no one received the least injury. I felt nothing this day of the noise and giddiness in my head, which had affected me in making the former experiments; and, whether from the force of habit, or any other cause, the shaking of our hands was less, and we felt less languor, though the heat had been so much more intense.

*XLVIII. A Proposal for Measuring the Attraction of some Hill in this Kingdom by Astronomical Observations. By the Rev. Nevil Maskelyne, B. D., F. R. S., and Astronomer Royal. p. 495.*

If the attraction of gravity be exerted, as Sir Isaac Newton supposes, not only between the large bodies of the universe, but between the minutest particles of which these bodies are composed, or into which the mind can imagine them to be divided, acting universally according to that law by which the force which carries on the celestial motions is regulated; namely, that the accelerative force of each particle of matter, towards every other particle, decreases as the squares of the distances increase; it will necessarily follow, that every hill must, by its attraction, alter the direction of gravitation in heavy bodies in its neighbourhood, from what it would have been from the attraction of the earth alone, considered as bounded by a smooth and even surface. For, as the tendency of heavy bodies downwards, perpendicular to the earth's surface, is owing to the combined attraction of all the parts of the earth on it, so a neighbouring mountain ought, though in a far less degree, to attract the heavy body towards its centre of at-

traction, which cannot be placed far from the middle of the mountain. Hence the plumb-line of a quadrant, or any other astronomical instrument, must be deflected from its proper situation, by a small quantity towards the mountain; and the apparent altitudes of the stars, taken with the instrument, will be altered accordingly.

It will easily be acknowledged, that to find a sensible attraction of any hill, from undoubted experiment, would be a matter of no small curiosity, would greatly illustrate the general theory of gravity, and would make the universal gravitation of matter as it were palpable, to every person, and fit to convince those who will yield their ascent to nothing but downright experiment. Nor would its uses end here, as it would serve to give us a better idea of the total mass of the earth, and the proportional density of the matter near the surface, compared with the mean density of the whole earth. The result of such an uncommon experiment, which I should hope would prove successful, would doubtless do honour to the nation where it was made, and the society which executed it.

Sir Isaac Newton gives us the first hint of such an attempt, in his popular *Treatise of the System of the World*, where he remarks, "That a mountain of an hemispherical figure, 3 miles high and 6 broad, will not, by its attraction, draw the plumb-line 2 minutes out of the perpendicular." It will appear, by a very easy calculation, that such a mountain would attract the plumb-line  $1' 18''$  from the perpendicular.

But the first attempt of this kind was made by the French academicians, who measured 3 degrees of the meridian near Quito in Peru, and who endeavoured to find the effect of the attraction of Chimborazo, a mountain in that neighbourhood, which is elevated near 4 miles above the sea, though only about 2 miles above the general level of the province of Quito. By their observations of the altitudes of fixed stars, taken with a quadrant of  $2\frac{1}{4}$  feet radius, they found the quantity of  $8''$  in favour of the attraction of the mountain, by a mean of their observations. This indeed was much less than they expected; but then it is to be considered, that their instrument was too small and imperfect for the purpose; and that they themselves were subject to great inconveniencies, being sheltered from the wind and weather by nothing but a common tent, and placed so high up the mountain as the boundary where the snow begins to lie unmelted all the year round. And indeed their observations, doubtless owing to these causes of error, differ greatly from each other, and are therefore insufficient to prove the reality of an attraction of the mountain Chimborazo, though the general result from them is in favour of it. Accordingly, one of the French gentlemen themselves, M. Bouguer, who drew up the account of their experiment, expresses his wishes, that a like experiment might be made, to find the

attraction of a mountain in France or England, where he thinks some might be found of sufficient bulk for the purpose. This experiment and these remarks were made in the year 1738, or above 30 years ago, yet I believe no similar experiment has ever been made in Europe.

I have made inquiries after a proper hill in this kingdom, for trying so curious an experiment, and have been informed of 2 places in particular, very convenient for the purpose. The one is situated on the confines of Yorkshire and Lancashire; where, within the compass of 20 miles, are situated 4 very remarkable hills; called Pendle-hill, Pennygant, Ingleborough, and Whernside, which have been estimated to be from 600 to 750 yards elevated above the plane of the vales between them. By calculation on these data it follows, that the sum of the contrary attractions of Whernside, the largest of these hills, on the plumb-line placed half-way up the hill, would not be less than 30", and might amount to 46", which it is evident is a very considerable quantity, and sufficient to give room to hope for a favourable and satisfactory success of the experiment. The other place pointed out for this purpose, is a valley 2 miles broad, between the hills Helwellin and Skidda, in Cumberland: which hills, according to a plan of them and the adjacent country, communicated by Mr. Smeaton, F.R.S., are elevated more than 1000 yards above the intermediate valley. By a calculation, made according to this plan, the sum of the contrary attractions of the plumb-line, placed alternately on the north side of Helwellin and the south side of Skidda, amounts to about 20", which is likewise a quantity large enough for the experiment. And though the density of the earth near the surface should be 5 times less than the mean density, as there is some reason to suspect, and the attractions, as here stated, should consequently be diminished in the proportion of 5 to 1, still the sum of the contrary attractions of Whernside would be 6" or 9", and the sum of the contrary attractions of Helwellin and Skidda would be 4"; which quantities are not too small to be measured and demonstrated by an accurate zenith sector, such as that belonging to the R. S., which I made use of at St. Helena, would be, if the fault in the suspension of the plumb-line, which I there discovered, was corrected, in the manner suggested in the Philos. Trans., vol. 54, p. 351.

*XLIX. An Account of Observations made on the Mountain Schehallien for finding its Attraction.\* By the Rev. Nevil Maskelyne, B. D., F. R. S., and Astronomer Royal. p. 500.*

In the year 1772, I presented the foregoing proposal, for measuring the at-

\* For this paper Dr. Maskelyne was honoured with the Society's gold medal. And the calculation of the earth's density, from these observations, amply confirmed the expectations and predictions of it; as fully appears in a future volume of this work.

traction of some hill in this kingdom by astronomical observations, to the R. S.; who, ever inclined to promote useful observations which may enlarge our views of nature, honoured it with their approbation. A committee was in consequence appointed, of which number I was one, to consider of a proper hill on which to try the experiment, and to prepare every thing necessary for carrying the design into execution. The Society was already provided with a 10-foot zenith sector made by Mr. Sisson, furnished with an achromatic object glass, the principal instrument requisite for this experiment, the same which I took with me to St. Helena in the year 1761; which wanted nothing to make it an excellent instrument but to have the plumb-line made adjustable, so as to pass before and bisect a fine point at the centre of the instrument. This was ordered to be done, and a new wooden stand provided for it, capable of procuring a motion of the sector about a vertical axis, by means of which it could be more easily brought into the plane of the meridian, or turned half round for repeating the observations with the plane of the instrument placed the contrary way, in order to find the error of the line of collimation. A large parallelpiped tent, 15½ feet square and 17 feet high, was also provided for sheltering both the instrument and the observer who should use it, composed of joists of wood well framed together, and covered with painted canvas. The Society was also possessed of most of the other instruments requisite for this experiment; as, an astronomical quadrant and transit instrument made by Mr. Bird, and an astronomical clock by Shelton, which had all been provided on occasion of the observations on the transit of Venus in 1761 or 1769. A theodolite of the best sort was wanting, a necessary instrument for obtaining the figure and dimensions of the hill. One of Mr. Ramsden's construction of 9 inches diameter, was thought the fittest for the purpose, on account of the excellence of the plan on which it was made, and the number of its adjustments, being capable of measuring angles for the most part to the exactness of a single minute. The other instruments prepared for this business were, 2 barometers of M. de Luc's construction, made by Mr. Nairne; a common Gunter's chain; a roll of painted tape 3 poles long, having feet and inches marked on it; 2 fir poles of 20 feet each, and 4 wooden stands, for supporting them when used in measuring the bases, and a brass standard of 5 feet, for adjusting them. The poles and stands were provided on the spot.

Though accounts had been received from various persons of several hills supposed proper for the intended purpose, some better and some worse authenticated; yet, in order to be sure of finding the best hill for the experiment, it was determined to send a person furnished with proper instruments, to make such observations on various hills in England and Scotland, as might enable us to choose the fittest for the purpose. Accordingly Mr. Charles Mason, who had been employed on several astronomical occasions by the R. S., was appointed to make



a tour through the Highlands of Scotland in the summer of the year 1773, taking notice of the principal hills in England which lay in his route, either in his going or in his return. It appeared from his observations, that scarcely any hill was so well adapted to the purpose as our sanguine hopes had led us to expect; for either they were not high enough, or not sufficiently detached from other hills, or their greatest length fell in a wrong direction, too near the meridian, instead of lying nearly east and west, which is a circumstance requisite to make a hill of a given height afford the greatest effect of attraction. In particular, the hills on the confines of Yorkshire and Lancashire, mentioned in the foregoing proposal, were found not to answer the description that had been given of them. Fortunately however Perthshire afforded a remarkable hill, nearly in the centre of Scotland, of sufficient height, tolerably detached from other hills, and considerably larger from east to west than from north to south, called by the people of the low country Maiden-pap, but by the neighbouring inhabitants, Schehallien; which, I have since been informed, signifies in the Erse language, constant storm; a name well adapted to the appearance which it so frequently exhibits to those who live near it, by the clouds and mists which usually crown its summit. It had also the advantage, by its steepness, of having but a small base from north to south; which circumstance, at the same time that it increases the effect of attraction, brings the two stations on the north and south sides of the hill, at which the sum of the two contrary attractions is to be found by the experiment, nearer together; so that the necessary allowance of the number of seconds, for the difference of latitude due to the measured horizontal distance of the two stations, in the direction of the meridian, would be very small, and consequently not subject to sensible error from any probable uncertainty of the length of a degree of latitude in this parallel. For these reasons the mountain Schehallien was chosen, in preference to all others, for the scene of the intended operations, and it was concluded to make the experiment in the summer of the year 1774.

It was foreseen that this experiment would be attended with considerable expence, and such as might easily have exceeded the common funds of the R. S., without some extraordinary assistance. The bounty of his majesty, our patron, happily removed this difficulty. At the humble request of the Society, his majesty had been graciously pleased to grant a very ample sum to their disposal, for defraying the expences of the observations of the late transit of Venus in 1769, as his majesty had before done with respect to the former transit of Venus in 1761. Out of this benefaction, after all expences had been paid, there was a considerable remainder; and, the Society humbly requesting to know his majesty's pleasure about the disposal of it, he was graciously pleased to direct them, to lay it out in such manner as they thought proper, and was most agreeable to

the end of their institution. As this bounty of his majesty had been originally granted for an astronomical purpose, the Society thought they could not dispose of it on any more important object, or in any manner more consistent with the intentions of their royal patron and benefactor, than by expending it on this astronomical experiment of the attraction of a mountain, as what could hardly fail of throwing light on the principle of universal gravitation, and was likely to lead to new discoveries concerning the constitution of this earth which we inhabit, particularly with respect to the density of its internal parts.

The experiment being thus resolved on, the next thing to be done was to fix on a proper person to carry it into execution. Numerous and interesting as my literary engagements are at the Royal Observatory, I had no thoughts of undertaking this care and labour myself, till the council of the R. S. were pleased to do me the honour to think my assistance necessary to insure the success of so important and delicate an experiment. Their thinking so was a sufficient motive with me to encounter whatever difficulties and fatigues might attend operations carried on in so inconvenient and inclement a situation. But it was requisite I should also have his majesty's permission for absenting myself so long from my duty at the Royal Observatory. This his majesty was graciously pleased to grant, and to allow me to stay as long as I thought necessary, to complete my very important observations. Such were the motives for undertaking this experiment, and the preparations made for putting it in execution. I am now to give an account of the operations themselves.

The quantity of attraction of the hill, the grand point to be determined, is measured by the deviation of the plumb-line from the perpendicular, occasioned by the attraction of the hill, or by the angle contained between the actual perpendicular and that which would have obtained if the hill had been away. The meridian zenith distances of fixed stars, near the zenith, taken with a zenith sector, being of all observations hitherto devised capable of the greatest accuracy, ought by all means to be made use of on this occasion: and it is evident, that the zenith instrument should be placed directly to the north or south of the centre of the hill, or nearly so. In observations taken in this manner, the zenith distances of the stars, or the apparent latitude of the station, will be found as they are affected by the attraction of the hill. If then we could by any means know what the zenith distances of the same stars, or what the latitude of the place would have been, if the hill had been away, we should be able to decide on the effect of attraction. This will be found, by repeating the observations of the stars at the east or west end of the hill, where the attraction of the hill, acting in the direction of the prime vertical, has no effect on the plumb-line in the direction of the meridian, nor consequently on the apparent zenith distances of the stars; the differences of the zenith distances of the stars taken on the

north or south side of the hill, and those observed at the east or west end of it, after allowing for the difference of latitude answering to the distance of the parallels of latitude passing through the two stations, will show the quantity of the attraction at the north or south station. But the experiment may be made to more advantage on a hill like Schehallien, which is steep both on the north and south sides, by making the two observations of the stars on both sides; for the plumb-line being attracted contrary ways at the two stations, the apparent zenith distances of stars will be affected contrary ways; those which were increased at the one station being diminished at the other, and consequently their difference will be affected by the sum of the two contrary attractions of the hill. On the south side of the hill, the plumb-line being carried northward at its lower extremity, will occasion the apparent zenith, which is in the direction of the plumb-line, continued backwards, to be carried southward, and consequently to approach the equator; and therefore the latitude of the place will appear too small by the quantity of the attraction; the distance of the equator from the zenith being equal to the latitude of the place. The contrary happens on the north side of the hill; the lower extremity of the plumb-line, being there carried southward, will occasion the apparent zenith to be carried northward, or from the equator; and the latitude of the place will appear too great by the quantity of the attraction. Thus the less latitude appearing too small by the attraction on the south side, and the greater latitude appearing too great by the attraction on the north side, the difference of the latitudes will appear too great by the sum of the two contrary attractions; if therefore there is an attraction of the hill, the difference of latitude by the celestial observations ought to come out greater than what answers to the distance of the two stations measured trigonometrically, according to the length of a degree of latitude in that parallel, and the observed difference of latitude subtracted from the difference of latitude inferred from the terrestrial operations, will give the sum of the two contrary attractions of the hill. To ascertain the distance between the parallels of latitude passing through the two stations on contrary sides of the hill, a base must be measured in some level spot near the hill, and connected with the two stations by a chain of triangles, the direction of whose sides, with respect to the meridian, should be settled by astronomical observations.

If it be required, as it ought to be, not only to know the attraction of the hill, but also from it the proportion of the density of the matter of the hill to the mean density of the earth; then a survey must be made of the hill, to ascertain its dimensions and figure, from which a calculation may be made, how much the hill ought to attract, if its density was equal to the mean density of the earth; it is evident, that the proportion of the actual attraction of the hill, to that computed in this manner, will be the proportion of the density of the hill to the mean density of the earth.

Thus there were three principal operations requisite to be formed. 1. To find by celestial observations the apparent difference of latitude between the two stations, chosen on the north and south sides of the hill. 2. To find the distance between the parallels of latitude. 3. To determine the figure and dimensions of the hill.

I arrived at the hill of Schehallien on the last day of June, and found the observatory and instruments there, which had been brought down some time before from London to Perth on board a ship, and thence conveyed over land to the hill under the care of Mr. Reuben Burrow, my late assistant at the Royal Observatory. The observatory was fixed half-way up the south side of the hill, as the place where the effect of the hill's attraction would be at the greatest, and it was placed in the like manner when it was afterwards removed to the north side. A circular wall was raised, 5 feet in diameter, and covered at top with a moveable conical roof for sheltering the astronomical quadrant; and a square tent was set up for receiving the transit instrument, all near the observatory. A bothie, or temporary hut, was also made near it, for my residence, while attending the astronomical observations on this side of the hill. I first put the sector, nearly in the meridian, by means of the variation compass; but, through the badness of the weather, which was almost continually cloudy or misty, I could not before the middle of July get a sufficient number of observations with the astronomical quadrant, to know the state of the clock, in order to draw a meridian line on the floor of the observatory, for setting the sector truly in the plane of the meridian. The first observations which I made with the sector, after it was set truly in the meridian, were on the 20th of July. Between this time and the end of the month, I observed the zenith distances of 34 stars, some to the north and some to the south of the zenith; and many of them several times over, having taken 76 observations in all, with the plane of the sector turned to the east. On the first of August I turned the plane of the instrument about, to face the west, and set it in the meridian again, by means of the meridian line drawn on the floor the 26th of July, and secured by piquets driven into the ground; and between that and the 15th of the same month, I observed 39 stars, including most of those taken in the former position of the instrument, and took 93 observations in all.

And here let me take notice of a method, which I fell upon, of verifying the position of the sector, with respect to the plane of the meridian, which, had I thought of it at first, would have saved me much trouble; and therefore I will now mention it, as it may be useful to future observers. It consists in observing the transits of two stars, differing considerably in declination from each other, across the vertical wire of the sector, and comparing the observed difference of their transits with the known difference of their right ascensions. If they agree,

it may be safely concluded, that the instrument is truly placed in the meridian. If not, by comparing the alteration that would be produced in the difference of the transits, by supposing the instrument out of the meridian, by any small quantity, as 1 degree or 10 minutes with the observed error, the deviation of the instrument from the meridian may be inferred. In this manner I found that the instrument had been set very exactly in the meridian, by means of the meridian line; the difference by the two methods coming out only 24 minutes of azimuth. As to the continuance of the instrument in the plane of the meridian, I had a constant proof of it by the same means, and also a further security, which I did not fail to attend to, by noting the degree and minute which an index depending on the vertical axis of the instrument pointed out on a fixed azimuth circle. Being apprehensive of error in an instrument supported on a wooden frame, I frequently examined the parallelism of the fore-arch to the back arch, by measuring their perpendicular distances at the two ends with a brass scale, whose vernier showed the 500th part of an inch, and found it liable to variations of a minute or two, owing probably to the force used in setting the sector to different zenith distances, and the weakness of some screws at the top of the frame; which small error I corrected, till I found it liable to continual returns: and I satisfied myself, that the plane of the sector never deviated above 3 minutes from the meridian, in any of the observations taken on the south side of the hill, which it is evident could not in the least affect the observed zenith distances of stars. I hardly ever observed, without examining the bisection of the point at the centre of the instrument, by the plumb-line; which was absolutely necessary, on account of the gradual changes of the wooden frame. My view in mentioning these minute circumstances, is to caution future observers, as well as to confirm my own observations. But whoever makes use of an instrument of this kind, supported on a wooden frame, will find the greatest attention necessary to attain the same degree of accuracy in his observations, as if his instrument was fixed to an immoveable wall. In the mean time, by observations taken with the quadrant and transit instrument, I got a meridian line, and planted a pole to preserve it on the top of the hill, to the south of the instrument, and another at the foot of the same hill; from which, by measuring off an equal distance to the east (as the south-west corner of the observatory lay to the east of the transit instrument) and setting up another pole, another meridian line was gotten, passing through the south-west corner of the southern station of the observatory. The reason for making the meridian line pass through the south-west corner of the observatory, rather than through the middle of it, was, that this part of it had been taken when the observatory had been used as an object in taking angles by the theodolite, in the survey of the hill.

While I was engaged in these astronomical observations, Mr. Burrow, at-

tended by Mr. William Menzies, a land surveyor in the neighbourhood, who had been recommended by some of the principal gentlemen of the country, as a proper person for this work, went out every day that the weather permitted, to take sections of the hill, and angles between several objects, for determining its figure and dimensions. The method made use of was this, which was proposed by Mr. Burrow, and was well adapted to the purpose. A number of station poles were set up at convenient distances all round the foot of Schehallien; but rather without its base, and chiefly on little eminencies rising from the foot of it, which formed a polygon of many sides, surrounding the hill; and when delineated on paper, show very nearly the shape of its base. At each station, the angular position of 2 or more of the other stations being observed with the theodolite, and one side being determined by means of a measured base, all the other sides will be known. From these stations, sections of the hill up to the top were taken in the following manner. The theodolite, being placed at any station, was pointed towards the hill; and a labourer was sent with a number of poles, which he was to plant in the ground truly upright, at regular distances and in a vertical plane, according to signals which he received from the person that stood at the theodolite, who also took the altitude of the foot of each pole, and the horizontal angle contained between the plane of the section poles and the next station pole to the right or left. The theodolite was then removed, and planted directly over the centre of this station pole, which was removed for this purpose; and the horizontal angle taken between a pole now planted at the first station and each of the poles of the section. The horizontal distance of the 2 station poles being known, the horizontal distance of each of the section poles from the first station, and their respective perpendicular altitudes above it, or depth below it, will be given.

It is manifest that these operations, when connected by angles with the 2 stations of the observatory and the meridian line, would at the same time give the shape and dimensions of the hill, and the distance of the parallels of latitude passing through the 2 stations of the observatory, as well as their respective elevations above the base of the hill. But errors being apt to accumulate in a long chain of triangles; to obviate this danger, as well as to produce a check on any great mistakes, that might happen to be made in reading off, or writing down, the angles, I caused a heap of stones, or cairn, as it is called by the people of the country, to be raised in a circular figure 6 feet high, at the highest point of the ridge of the hill, which is to the west of it, as a signal to be observed from the several angles of the polygon, and as a means of connecting the 2 stations of the observatory by a smaller number of triangles. Another cairn towards the eastern end of the ridge of the hill was afterwards set up for the like purpose. I proposed to determine the distance of the 2 cairns by connecting

them by angles with a base, to be measured in a level spot in the vale below the hill, and then to make use of the said distance as a secondary base for determining the sides of the polygon, and the distance of the 2 stations of the observatory. Had the 2 cairns been visible from the 2 stations of the observation, 2 triangles would have sufficed for connecting the 2 stations together. But notwithstanding that this was not the case, and that only the 2 cairns were visible from each other, yet all the angles of these 2 triangles were measured by Mr. Burrow in the following method, suggested by himself. He went with the theodolite to the neighbouring hill on the south side of Schehallien, which runs parallel to it; and, by varying his situation, found a point whence the western cairn and southern observatory appeared by the theodolite to be in one vertical plane; and, removing the theodolite, he planted a pole there. In like manner he planted another pole on the same hill, in a vertical plane with the southern observatory and eastern cairn. Then returning to the observatory, he took the horizontal angle contained between the 2 poles, which it is evident is equal to its opposite angle, or that contained between the cairns. And going to the west cairn, he took the angle contained between the east cairn and the pole planted on the opposite hill, in a line with the southern observatory and west cairn, which is the same with the angle between the east cairn and southern observatory. And lastly, going to the east cairn, he took the angle contained between the western cairn and the pole placed on the opposite hill in a line with the east cairn and southern observatory. Thus were the 3 angles found of the triangle made by the southern observatory and 2 cairns. In the like manner were the angles of the triangle made by the northern observatory and 2 cairns found afterwards. And, as a proof that the angles of the 2 triangles were rightly determined, their sum in the first case differed from  $180^\circ$  by little more than 2 minutes; and in the second case by only half a minute.

Notwithstanding the advantages which attended this method of finding the distance of the 2 stations of the observatory, I thought it proper to make use also of the other method of doing the same thing by a small number of triangles, carried directly across the hill, thinking it expedient, in a matter of such consequence, to rely on no single operation; but, as far as possible, to confirm every deduction by another found in an independent manner. I had caused 2 poles to be set as far up the hill of Schehallien as they could be placed; one as near the western, and the other the eastern cairn, as they could be, so as to be visible from the southern station of the observatory: also 2 others in like manner visible from the north observatory; one of which was very near the east cairn, and the other only 269 feet distant from the westernmost of the 2 poles visible from the south observatory; so narrow was the ridge of the hill in that part, though it grew wider both to the west and east, but much more

towards the latter. With these 4 poles, the east cairn, and the 2 stations of the observatory, 5 triangles were formed, connecting the 2 stations of the observatory, the relative situation of which to each other would be determined as soon as the length of any one of the sides of these triangles was known, either by comparing it with a base measured in the valley below, or with the distance of the 2 cairns settled in that manner.

I had got sufficient observations of zenith distances of stars with the sector on the south side of the hill by the 15th of August; I prepared therefore for removing the observatory and instruments to the new station on the north side. This was a work of great labour and difficulty, as every thing was carried over the ridge of the hill on men's shoulders, and some of the packages were very weighty; it employed the labour of 12 men for a week, and was completed on the 26th. A large level area had been cut away, with great labour, here, in the side of the hill, for receiving the observatory, as had before been done on the south side of the hill. A new bothie was also erected, and places for holding the quadrant and transit instrument, as before, adjoining to the observatory.

The badness of the weather prevented me from beginning my observations with the sector till the 4th of September; but, that being a clear night, I had a fair opportunity of putting in practice the method of bringing the instrument into the meridian by the transits of the stars across the plane of the sector, before-mentioned. The sector being put up with its plane facing the west, and set near the meridian by the variation compass, allowing for the variation, I found, by the transit of  $\alpha$  Draconis, on the north side of the zenith, compared with those of  $\alpha$ ,  $\beta$ , and  $\theta$  Cygni on the south side, that the instrument deviated  $49\frac{1}{4}$  minutes to the west of the south in azimuth; which being corrected, by turning the instrument about on its vertical axis, towards the east, by the help of the divisions on the azimuth circle; I then found by the transit of  $\gamma$  Cephei, compared with that of  $\pi$  Cygni on the south side, that the instrument deviated 7 minutes to the east of the south in azimuth, which I corrected accordingly. And so near was it brought to the meridian in this manner, that by the most exact comparison of the transits of several stars on the 7th and 8th instant, it appeared to be only 2 minutes out of the meridian, and that to the east of the south; which small error I also attempted to correct; the instrument rested 1 minute out of the position which I intended to give it, owing to the difficulty of turning it about to such great nicety, and so I let it remain.

It was indeed a most fortunate circumstance, that I thus got the instrument so near the meridian by the very first night's observations, those of September 4th; for the badness of the weather in the day prevented me from getting a meridian line by the sun till the 15th. Had I therefore been obliged to wait



for setting the instrument by the sun, I should have lost 4 good days observations, which were  $\frac{1}{3}$  of those I took on this side of the hill, with the plane of the instrument turned to the west, and been retarded near 3 weeks in my observations; and, as the opportunities of weather fit for observing at all were but very rare, I might have been thrown back into the winter, and defeated of making so complete a set of observations on the north side of the hill as I had got on the south side, whose correspondence would thus have been rendered less perfect. I had the satisfaction, however, when I drew the meridian line on the floor of the observatory, by the equal altitudes of the sun taken on the 15th, to find it agree perfectly, even to the same minute, with the position of the instrument, as determined by the transits of the stars. But no one will doubt of the superior ease and readiness afforded by the latter method in preference to the other.

On the 20th of September I completed the observations with the plane of the sector, turned to the west, having observed 32 stars, and taken 68 observations in all. On the 22d, I turned it about with the plane to face the east, and set it again in the meridian, by putting it parallel to its former position, by means of the meridian line secured by marks made on picquets let into the ground perpendicularly below the plane of the instrument, before it was turned. Between this time and the 24th of October, I observed 37 stars, and took 100 observations in all, with the plane of the instrument facing the east: and thus I completed my whole series of observations with the sector, having observed 43 different stars in all, on both sides of the hill, and taken 337 observations.

As a few observations, taken with so excellent an instrument as this zenith sector, would have been sufficient to determine the apparent difference of latitude of the 2 stations of the observatory, to a second or 2; I am apprehensive I may be thought by many to have multiplied observations unnecessarily. However that may be, I apprehend, that doubling the observations in each station of the observatory, by taking them with the plane of the instrument alternately facing the east and west, will be allowed to be a proper step, as the line of collimation of the instrument is thus separately determined at each station, and so all danger of any alteration happening in the same, in its removal from one side of the hill to the other, is entirely obviated. I had indeed all the reason in the world to think, that the sector was carried from one station to the other without the least accident: but still it was proper to guard against what was possible to happen.

But I had reasons also for multiplying the observations made in the same position of the instrument. It was important to demonstrate the exactness of the instrument from the near agreement of a number of observations taken with it, as its excellence was not to be entirely presumed, unless this proof could be

shown in its favour. Besides, it might be expected that some unsteadiness or warping of the wooden stand, on which it was supported, might affect the accuracy of the observations; or there might be variable and discordant refractions, even near the zenith, on the side of so steep a hill, more than are found in lower situations. Add to this, that when I began my observations on the south side of the hill, having a prospect of bad weather before me, and not knowing how few observations I might be able to get on either side of the hill, I thought it prudent to endeavour to observe most of the stars in the British catalogue, which came within the reach of the instrument, that I might be sure of being provided with observations of some at least of the same stars, which I might afterwards observe when I should be removed to the north side of the hill; where, after an interval of perhaps some months, many stars, that before passed the meridian in the night, would pass it in the day, and consequently be either invisible through the telescope of the sector, or more precarious of being seen.

Though a meridian line had been found by the transit instrument at the south observatory, by which the relative situation of the 2 stations of the observatory, as well as of the other points of the hill, with respect to the meridian, might be determined; yet I judged it would be more satisfactory to confirm this by another meridian line drawn at the northern observatory. This I found, as I had done the former, by setting the transit instrument to agree with the pole star at the computed time of its passing the meridian, and confirmed it by comparing the difference of the transits of the pole star and of  $\alpha$  Pegasi,  $\alpha$  Andromedæ, and  $\gamma$  Pegasi, with their difference of right ascension, in the same manner by which I had put the sector in the plane of the meridian, and found it to agree with the former meridian line within 2 minutes.

It remains to give an account of the manner in which the two bases were measured; one in a level spot at the foot of the hill, to the southward; and the other at the distance of about  $2\frac{1}{4}$  miles from the hill to the north west, in the plain of Rannoch. I caused 2 measuring poles to be made of straight grained well seasoned fir, in the form of square tubes, 3 inches square and 20 feet long, and strengthened with square pieces within side at several distances. These were carefully compared with the brass standard made by Mr. Bird, the same which was used in the measure of the degree at Pennsylvania, immediately before they were applied to the measure of the bases, and the height of the thermometer noted at the time, in order to make allowance for the expansion or contraction of the brass standard by heat or cold. Four wooden stands were provided for supporting the poles; each having a triangular base with 3 iron spikes beneath, at each of the angles. An upright pole, 6 feet high, rose from the middle of one side of the triangle, and 2 short braces were joined to it

from the 2 ends of this side, and a long slant pole from the opposite angle. Two sliding arms were put on the upright pole, capable of being raised or depressed, one above and the other below the place where the slant pole was fastened to the upright pole, for supporting the measuring poles at a convenient height above the ground. In measuring the base, one end of a pole was supported on one of the stands, and the other end on another stand; and it was set horizontal by means of a spirit level laid on it about the middle, and by raising or depressing the arm on which it rested at one or the other end. The other pole was then, in like manner, supported on the 2 other stands truly level, and in the same vertical plane with the former pole, namely, that of the intended base, without regarding whether they were exactly of the same height, and with some small horizontal interval between their ends. This interval was measured by laying one leg of a brass rectangle, which was divided into inches and tenths, along one pole, while the other, or vertical leg, touched the end of the other pole; for it was not thought advisable, to bring the ends of the poles to touch exactly, as that would have taken up a great deal of time, and might have endangered the altering the position of the hindermost pole, if it should chance to receive any shock by laying down the foremost pole. It is evident that the inches and tenths given by the divisions of the brass rectangle are to be added into one sum together with the poles, in computing the length of the base. When the foremost pole was truly placed, and the interval between them had been measured by the divided side of the brass rectangle, the hindermost pole was taken up, and the stands on which it had rested were advanced forwards, and the pole again laid on them, truly level, and in the true direction of the base. In order to set the poles continually in the proper direction of the base, the following method was used. The theodolite was first set up at one end of the base, and an upright pole at the other, and another in the middle, and a third was from time to time advanced to a little distance forward; and the measuring poles were sometimes placed in the proper direction by the eye, looking along the lengths of both poles together to the upright pole before them, and sometimes by the help of the theodolite. In this manner, about the middle of September, a base was measured by Mr. Burrow and Mr. Menzies of 3012 feet, in the valley at the foot of the hill to the south west; but not so accurately as this method is capable of, owing to the stands being very unsteady, through the looseness of the spikes in the feet and other faults, during the measuring the first quarter of the base, though they were mended before the mensuration of the remainder of it. The mensuration of another base of the length of 5897 feet, in the meadow of Rannoch, about 24 miles to the north-west of the centre of the hill, which I attended myself, was performed with the greatest accuracy, according to the same method, on the 10th, 11th, and 12th of Oct., with new stands, more substantial and firm than the former.

The extreme badness of the weather, no less retarded the operations of the survey than the celestial observations; for there was almost constant rain, mist, or high wind, to obstruct the use of the theodolite: indeed all the people of the country agreed, it was the worst season that had ever been known. So that it was not till the 20th of October that the sections had been carried all round the hill. Nor would this work have been so much forwarded as it was, had it not been for the use of an additional theodolite of the same construction, and by the same maker, as the former, which was lent me, on my request, by the right honourable James Stuart Mackenzie, lord privy seal for Scotland; who, having long cultivated a distinguished taste for astronomy, was pleased to honour the experiment of attraction with every assistance which his interest or recommendation could procure. I am particularly to acknowledge the favour he conferred on me, by introducing me to the acquaintance of Sir Robert Menzies, Baronet, his brother-in law, a gentleman conversant in mathematical and philosophical learning, who honoured me with his friendship during my residence in the country; and, besides many personal civilities shown to myself, rendered many material assistances to the main purpose of carrying on the experiment. It is with pleasure also, that I acknowledge the civilities of all the neighbouring gentlemen, who often paid me visits on the hill, and gave me the fullest conviction that their country is with justice celebrated for its hospitality and attention to strangers. I was honoured also by visits from many learned gentlemen who came from a great distance; particularly the lord privy seal, Dr. Wilson, professor of astronomy at Glasgow, and his son, and Dr. Reid, professor of moral philosophy, and Mr. Anderson, professor of natural philosophy, also at Glasgow, Lord Polwarth, Mr. Ramsay, professor of natural history at Edinburgh, Mr. Commissioner Menzies, of the customs at Edinburgh, Mr. Copland, and Mr. Playfair, of the university of Aberdeen, the Rev. Mr. Brice, and my esteemed friend Col. Roy, who had been my companion in the journey as far as Edinburgh. So great a noise had the attempt of this uncommon experiment made in the country, and so many friends did it meet with interested in the success of it!

The use of the 2 theodolites at once, as mentioned above, much forwarded the completing of the sections all the month of October; Mr. Menzies observing the bearings at one station with one theodolite, while Mr. Burrow observed the altitudes or depressions with the other theodolite at the other station; and the labourer, who used to plant the poles in the hill, taking only one with him, and fixing it up at one place to be observed at both theodolites, and then removing it to the next station for the like purpose. Notwithstanding which, the weather became at length so bad, by the early coming in of frost and snow in the beginning of November, when the survey was nearly completed, as to

render it impossible to do any thing more that season. It became therefore necessary to finish this astronomical campaign, leaving the theodolite in the care of Mr. Menzies, to complete what little remained to be done the next season.

I have thus described the plan which was adopted for the operations on Schehallien, and the manner in which it was carried into execution; it only remains to give the result of computations made on those operations for deducing the effect of the attraction of the mountain. The operations themselves at large shall be communicated at another opportunity.

I had caused the arch of the sector to be divided by fine points, according to a new and arbitrary division adapted to the method of continual bisection. One 8th part of the radius of the instrument was found by 3 bisections, and applied as a chord to the arch from the middle on each side, intercepting each way  $7^{\circ} 9' 59''.917$ . These spaces were each divided by points into 128 parts, by continual bisection; therefore one division will contain  $3' 21''.561854$ . Hence the number of degrees and minutes, answering to any number of divisions, may easily be found. Twenty-four additional parts were also set off, taken from the former, and added at the 128th division on each side, to fill up the whole extent of the arch, which thus consisted of 152 divisions on each side of the centre, answering to an angle of  $8^{\circ} 30' 37''.4$ , which was therefore the greatest angle the instrument was capable of measuring. To find the value of the parts of the micrometer in seconds, I measured the distance of the points on the limb, by 5 at a time, by means of the plumb-line, in parts of the micrometer from 0 to 128, on each side of the middle or point marked 0 on the arch. By a mean of all these measures, one division of the arch, or  $3' 21''.562$ , came out equal to 4 revolutions and 34.8272 parts of the micrometer, 41 of which make one revolution; and therefore one part is equal to  $1''.0137545$ , and 41, or one revolution, is equal to  $41''.5639345$ . Hence the value of any number of revolutions and parts of the micrometer may be easily found. At all observations of the same star, whether on the north or south side of the hill, I brought the same point of the arch, namely, that which agreed nearest with the zenith distance of the star, under the plumb-line, so that the difference of the apparent zenith distances of the same star, on contrary sides of the hill, is given in parts of the micrometer, and has no reference to the divisions of the instrument, whether they be equal or unequal; and, the parts of the micrometer screw being perfectly equal, as I had formerly satisfied myself by measuring the interval of 2 given points on the arch with different parts of the screw, that difference of apparent zenith distances may be perfectly relied on, as far as depends on the instrument, provided the bisection of the star by the wire in the telescope, and that of the point on the arch by the plumb-line, were accurately performed. As the plane of the instrument was placed both east and west, at both stations of

the observatory, the difference of the latitude of the 2 stations may be found as well from the observations made in one position of the instrument, as the other. If the instrument had suffered no change by being carried over the hill, that is if the line of collimation was not altered by it, the results should come out equally true from the observations taken in both positions of the instrument. On the contrary, if the line of collimation should, by any means, have suffered any alteration between the observations made at the 2 stations, this would cause the difference of latitude to appear too small, by the observations made in one position of the instrument, by the quantity of the alteration, and as much too great, in the other position of the instrument. But still the mean between the 2 results, deduced from the observations taken in the 2 different positions of the instrument, would give the true difference of latitude; and that equally, whether the line of collimation had suffered any change or not. Therefore this will be the best method of comparing the observations together, and I shall take a mean of all the results, deduced from the observations taken in each position of the instrument separately, and then a mean of those means for the true difference of latitude. By single observations of 10 stars; viz.  $\beta$ ,  $\alpha$ , and  $\gamma$  Cassiopeæ, and  $\iota$ ,  $\eta$ ,  $\beta$ , 39, 45, 46, and 53 Draconis, made on both sides of the hill, with the plane of the sector facing the west, after making the proper allowance for precession, aberration, and deviation, and semi-annual solar nutation of the earth's axis (see my tables annexed to my Observations made at the Royal Observatory,) the apparent difference of latitude between the 2 stations of the observatory, comes out  $54''.1$ ,  $54''.7$ ,  $54''.0$ ,  $55''.4$ ,  $55''.0$ ,  $55''.0$ ,  $52''.2$ ,  $54''.0$ ,  $54''.3$ ,  $53''.1$ , respectively; the mean of all which is  $54''.2$ ; the greatest difference of any one result from the mean being only  $2''$ . In like manner, by single observations of as many stars; viz.  $\beta$  and  $\alpha$  Cassiopeæ;  $\epsilon$  Ursæ majoris;  $\beta$ , 39, 46, 0, 49, and 53, Draconis; and 23 Cygni; made on both sides of the hill, with the plane of the sector facing the east; after making all the allowances as before, the apparent difference of latitude comes out  $54''.5$ ,  $52''.3$ ,  $56''.8$ ,  $53''.5$ ,  $54''.5$ ,  $57''.2$ ,  $56''.1$ ,  $55''.3$ ,  $54''.1$ ,  $55''.1$ , respectively; the mean of all which is  $55''$ ; the greatest difference of any one result from the mean being  $2''$ . The two means  $54''.2$  and  $55''.0$  differ only  $0''.8$ , which should argue only an alteration of  $0''.4$  in the line of collimation; but this is too small a quantity to be depended on; and therefore it is most probable, that the state of the instrument remained unvaried. However, whether it did or not, the mean of the 2 means, or  $54''.6$ , is to be esteemed the apparent difference of latitude between the 2 stations of the observatory, and, when compared with the difference of latitude which should result from the trigonometrical measures, will give the sum of the 2 contrary attractions of the hill. It must be owned, that this point will be settled with rather more certainty, when all the observations made with the sector,

which exceed 300, shall have been computed; but, as from the agreement of these results together, as well as from the small differences that are usually found in observations made within a few days of one another, we may presume, that the result from the whole will not differ materially from that deduced above from 40 observations, I thought I had better take this opportunity of gratifying the impatience of the society in presenting them with these my first computations, before their summer recess, than delay giving them any account at all of this experiment, till I had leisure to complete the whole of my calculations.

I am now to show, what the distance is between the parallels of latitude passing through the 2 stations of the observatory in feet, according to the trigonometrical mensuration; and thence, what the difference of latitude ought to have been, if the hill had been away, or had exerted no sensible attraction. This depends on the enumeration of several particulars. The length of the base measured in the meadow of Rannoch, was 5897.119 feet, according to the state of the brass standard, when the thermometer was at  $40^{\circ}$ ; but, to reduce it to answer to the state of the brass standard in the heat of  $62^{\circ}$ , we must subtract 16.721 feet; we should also subtract further 0.327, for the diminution which the brass standard has suffered by wear, and there remains 5880.071 feet for the true length of the base in Rannoch. See Phil. Trans. vol. 58, p. 313, 324, 326. Hence, with the help of the angles taken with the theodolite at the ends of the base in Rannoch, and at the west cairn, the horizontal distance between the east and west cairns comes out 4047.4 feet. Nearly the same result comes out from the base measured on the south side of the hill, though with less exactness; this, when all corrections are made, is 3011.684 feet, whence the distance of the 2 cairns should come out 4058 feet, or about 10 feet longer than results from the base in Rannoch. But I prefer the deduction from the base in Rannoch as most to be depended on. Hence, by the calculation of the 2 triangles formed by the 2 cairns and the two stations of the observatory, the distance between the parallels of latitude passing through the 2 stations comes out 4364.4 feet, which, according to M. Bouguer's table of the length of a degree in this latitude of  $56^{\circ} 40'$ , at the rate of 101.64 English feet to one second, answers to an arc of the meridian of  $42''.94$ . The other series of triangles carried across the hill, gives the same distance of the parallels only 10 feet less, and consequently the arc of the meridian only  $\frac{1}{10}$  of a second less. Thus the difference of latitude found by the astronomical observations, comes out greater than the difference of latitude answering to the distance of the parallels, the former being  $54''.6$ , the latter only  $42''.94$ . The difference  $11''.6$  is to be attributed to the sum of the 2 contrary attractions of the hill.

The attraction of the hill, computed in a rough manner, on supposition of its density being equal to the mean density of the earth, and the force of at-

traction being inversely as the square of the distances, comes out about double this. Whence it should follow, that the density of the hill is about half the mean density of the earth. But this point cannot be properly settled till the figure and dimensions of the hill have been calculated from the survey, and thence the attraction of the hill, found from the calculation of several separate parts of it, into which it is to be divided, which will be a work of much time and labour; the result of which, will be communicated at some future opportunity.

Having thus come to a happy end of this experiment, we may now consider several consequences flowing from it, tending to illustrate some important questions in natural philosophy. 1. It appears from this experiment, that the mountain Schellien exerts a sensible attraction; therefore, from the rules of philosophizing, we are to conclude, that every mountain, and indeed every particle of the earth, is endued with the same property, in proportion to its quantity of matter.

2. The law of the variation of this force, in the inverse ratio of the squares of the distances, as laid down by Sir Isaac Newton, is also confirmed by this experiment. For, if the force of attraction of the hill had been only to that of the earth, as the matter in the hill to that of the earth, and had not been greatly increased by the near approach to its centre, its attraction must have been wholly insensible. But now, by only supposing the mean density of the earth to be double to that of the hill, which seems very probable from other considerations, the attraction of the hill will be reconciled to the general law of the variation of attraction in the inverse duplicate ratio of the distances, as deduced by Sir Isaac Newton from the comparison of the motion of the heavenly bodies with the force of gravity at the surface of the earth; and the analogy of nature will be preserved.

3. We may now therefore be allowed to admit this law; and to acknowledge, that the mean density of the earth is at least double of that at the surface, and consequently, that the density of the internal parts of the earth is much greater than near the surface. Hence also, the whole quantity of matter in the earth will be at least as great again as if it had been all composed of matter of the same density with that at the surface; or will be about 4 or 5 times as great as if it were all composed of water. The idea thus afforded us, from this experiment, of the great density of the internal parts of the earth, is totally contrary to the hypothesis of some naturalists, who suppose the earth to be only a great hollow shell of matter; supporting itself from the property of an arch, with an immense vacuity in the midst of it. But were that the case, the attraction of mountains, and even smaller inequalities in the earth's surface, would be very great, contrary to experiment, and would affect the measures of the degrees of the meridian.



much more than we find they do; and the variation of gravity in different latitudes, in going from the equator to the poles, as found by pendulums, would not be near so regular as it has been found by experiment to be.

4. The density of the superficial parts of the earth, being however sufficient to produce sensible deflections in the plumb-lines of astronomical instruments; will thus cause apparent inequalities in the mensurations of degrees in the meridian; and therefore it becomes a matter of great importance to chuse those places for measuring degrees, where the irregular attractions of the elevated parts may be small, or in some measure compensate one another; or else it will be necessary to make allowance for their effects, which cannot but be a work of great difficulty, and perhaps liable to great uncertainty.

After all, it is to be wished, that other experiments of the like kind with this were made in various places, attended with different circumstances. We seldom acquire full satisfaction from a single experiment on any subject. Some may doubt, whether the density of the matter near the surface of the earth may not be subject to considerable variation; though perhaps, taking large masses together, the density may be more uniform than is commonly imagined, except in hills that have been volcanos. The mountain Schehallien however bears not any appearance of having ever been in that state; it being extremely solid and dense, and seemingly composed of an entire rock. New observations on the attraction of other hills, would tend to procure us satisfaction in these points. But whatever experiments of this kind be made hereafter, let it be always gratefully remembered, that the world is indebted, for the first satisfactory one, to the learned zeal of the R. S. supported by the munificence of George the Third.

Tables are then added of all the zenith distances of the several fixed stars, that were observed, at the two observatories, from which was deduced the preceding quantity of the celestial arc, answering to the geographical distance between the parallels of latitude passing through the two observatories.

END OF THE SIXTY-FIFTH VOLUME OF THE ORIGINAL.

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*I. On the Nature of the Gorgonia; that it is a real Marine Animal, and not of a Mixed Nature, between Animal and Vegetable. By John Ellis, Esq. M. D., F. R. S. In a Letter to Daniel Solander, M. D., F. R. S. Anno 1776, Vol. LXVI.*

It was your particular request, before you went to the South Seas, that I should continue my researches into the formation and growth of Zoophytes, more particularly of those formerly called Ceratophytos, now Gorgoniæ; and known in English by the name of sea-fans, sea-feathers, and sea-whips, to which class

the red coral should be added. This you thought the more necessary, as the accounts already published of them by Dr. Linnæus and Dr. Pallas seemed to make them of a mixed nature in their growth, between animals and vegetables: a thing not easily to be reconciled to the usual operations of nature. I was so fortunate about that time to receive a most excellent collection of different species of these animals, preserved at the sea side in spirits, at Dominica, which has enabled me to show more clearly, that they are true animals, growing up in a branched form, and in no part vegetable.

From the following observations it will appear, that the gorgonia is an animal of the polype kind, resembling the common fresh water polype in many of its qualities, but differing from it in the remarkable circumstance, of producing from its own substance a hard and solid support, serving many of the purposes of the bone in other animals. Every one knows, that the common polype sends out its young from its side, like buds, which being grown to the form of the parent animal, to which they still adhere, send out again their own young, like buds, adhering to themselves; and this is repeated, till at length the whole acquires a branched appearance, resembling a vegetable; see fig. 1, pl. 15. The gorgonia grows nearly in the same manner; and hence arises its resemblance to a shrub, which has given occasion to the mistake of placing it in the vegetable kingdom. But though the nature of these animals is so much like the polypes, they differ in several circumstances; the most remarkable is that already mentioned, the hard bone by which the gorgonia is supported. This is not formed by any kind of vegetation, but by a concreting juice thrown out from a peculiar set of longitudinal parallel tubes, running along the internal surface of the fleshy part. In the coats of these tubes are a number of small orifices, through which the osseous liquor (if I may use the expression) exudes; and concreting, forms the layers of that hard part of the annular circles, which some, judging from the consistence rather than the texture, have erroneously denominated wood.

Dr. Pallas, in his *Elench. Zoophytorum*, p. 162, is of opinion, that the layers of which the wood, as he calls it, of the tougher gorgoniæ is composed, may be separated into numerous longitudinal fibres; that the longitudinal striæ, which frequently appear on its external surface, are owing to this structure; and that these fibres are in fact hollow, like the wood of trees, the cavity of the tubes being closed up, as they become hard and rigid.

I was nearly of the same opinion when I was writing my *Essay on Corallines*, as may be seen in the *Philos. Trans.* vol. 48, p. 18, and also p. 504, where I have compared the herring-bone coralline, which is composed of many little tubes, to the growth of sea-fans and sea-feathers, now called gorgoniæ; and likewise in my *Observations on the Growth of the red and white Coral*, see

Philos. Trans., vol. 48, p. 504 ; but experience has since fully convinced me of the contrary : for on the strictest examination with the microscope, of the internal horny parts of several of those gorgoniæ fresh from the sea, and immediately preserved in spirits, not the least appearance of tubes within the horny part can be discerned, either in the longitudinal or transverse sections. There is indeed a regular cannulated appearance on the surface ; but this seems to be only an external moulding, and not formed by a series of longitudinal tubes with interstices, as in plants ; nor is it difficult to explain whence such a moulding may arise. I have observed, that the inner surface of the fleshy part contiguous to the bony or horny part, is furnished with longitudinal parallel tubes, which through certain pores supply the osseous matter ; this, being soft at first, and only afterwards becoming hard, so as necessarily to take the form of the concave surface by which it is closely pressed, and therefore assumes a striated appearance. This is plainly seen in fig. 2, A, where the ends of the tubes and the striated appearance on the gorgonia flabellum are expressed ; -and at fig. 2, B, two of them are magnified.

In the *isis hippuris*, or black and white jointed coral, which is very nearly a-kin to this genus, these tubes are still more clearly to be seen, as they are larger, and the channels much deeper ; see fig. 3, where A is a part of the coral of its natural size ; B is an extremity of one of the branches magnified, with the bony part laid bare ; C a part of the same, with the bony part taken out, to show the tubes with their internal orifices, through which the osseous juice is supposed to exude, and form the layers of the bony and horny part. This formation of the hard part, or bone of the stem, seems to be a principal use of the longitudinal tubes ; but they have another also, of great consequence in the growth of the gorgonia : for it is by means of these, that the animal spreads itself downwards over the substances which serve for its basis, thence deriving a firmness proportioned to its bulk. By means of these likewise it repairs any deficiencies arising either from accident or natural decay, by which the life of the whole would be endangered. At fig. 2, C, D, the broken stem in the gorgonia flabellum is strengthened and made firm by the lateral reticulations being covered over with the horny substance by means of these fleshy tubes and polype suckers. This is very different from any natural repairs of broken or wounded branches in trees. Besides, these tubes extend themselves any way, creeping over every substance which may serve for their support and preservation of the animal, throwing out the horny or osseous juice to make the whole texture firmer. This wonderful contrivance of nature is certainly instinct in this low order of animals. To give a better idea of this kind of instinct, and to show in what it differs from what is called radication in plants, with which some people, for want of better information, are apt to confound it, I have given a

figure of the manner in which the flustra foliacea fastens itself to shells; see fig. 4. This figure is a little magnified, to show the form of the cells, as they have spread themselves over the surface of the scollop-shell. The advocates for the vegetation of zoophytes, I hope, will be convinced, that the part that sticks to the shell is not a root, but only a single course or layer of cells of the same animal. As it rises into leaf-like branches they become double, or 2 layers of cells, placed in such an opposition to one another as to strengthen the whole, like the cells in the honey-comb; and what is very singular, the narrow part of the stem near the shell, often consists of 4 or more layers of cells, which the animal, by this kind of instinct, most certainly applies to strengthen that slender part against the force of the waves. For another instance of the base of a zoophyte spreading downwards to secure itself, we have an example in the madrepora muricata, which is extending itself over a dead animal of the same species, as in fig. 5.

The following remark of Dr. Pallas will show, that as he conceives the wood or horny stem to be composed of tubes, so he thinks that there is a communication of juices from the polypiferous pores on the cortical part, to the inside or horny part, as in trees: for he observes, that as the trunk of the gorgonia is always proportioned to the size of its branches, the wood or horny part of the trunk, notwithstanding its hardness, must necessarily thrive, grow, and increase every way, even though the organs of the bark, or surrounding fleshy substance, at the trunk and base are obliterated; and hence he concludes, that the trunk must receive nourishment from the branches, and apprehends, this nourishment to be absorbed and prepared by polypiferous pores. Now it is evident, that the idea of the trunk and base of a tree growing in thickness, when it is divested of its surrounding bark, is contrary to the known laws of vegetation. The only method of increase in the trunks of trees is by the apposition of new layers from the bark, which cannot be produced but while the bark is subsisting.

Nor can the gorgonia increase in size, in those parts where it is deprived either of the flesh with the polype suckers, or the surrounding fleshy tubes, which communicate with these suckers; for these suckers and tubes are the organs that prepare and deposit the several thin layers, which form the support or bony part, here called wood, as I have shown before. If, on examining the internal structure of these zoophytes, it were found, that their growth and fabric anywise resembled that of vegetables, this would indeed afford a presumptive argument, that they did participate of a vegetable nature. Yet even in that case, it would be much more reasonable to suppose them animals of the lowest order, raised but one degree above the vegetable tribe, than to conjecture a monstrous metamorphosis repugnant to the general analogy of nature. But the truth is, that though the hard parts of many gorgoniæ have very much the

external appearance of wood, yet the internal structure differs in the most essential points from vegetables.

In order to prove this, I have compared different sections of the gorgonia with correspondent sections both of sea and land plants, and find they differ in the following particulars: the longitudinal sections of the stems of the larger fuci, such as the *fucus digitatus*, *esculentus*, *nodosus*, and *saccharinus*, appear composed of parallel jointed tube-like figures, the joints of which are composed of gland-like cells; these tubular appearances, when highly magnified, are discovered to be connected together by transparent reticulated fibres, or very minute transverse tubes, interwoven with the upright ones. In a horizontal section, the ranges of cells, which look like rays from the centre, as they approach the bark, become smaller and smaller, and most probably correspond with the minute pores which cover the outer surface of the plant; for when the sides of the dry stems are soaked in water, they quickly imbibe it, and soon become full of a gelatinous liquor; all which is totally different from the texture of the gorgonia.

We come now to compare them with land plants, such as shrubs, like to which they are generally supposed to grow. The gorgonia has no regular series of hollow fibres or little tubes, in what is called the wood, either longitudinal or horizontal. It appears composed of a sort of irregular laminæ like horn; the fibres of which take no certain direction, nor preserve in any two places the same thickness. It has no series of utricular vessels, as the transverse vessels of wood are called by Malpighi; or insertions as they are called by Dr. Grew. These are essentially necessary, as forming a communication from the bark and the internal parts of the wood quite through. On the contrary, the concentric circles of the gorgonia have no connection with each other; they run like so many parallel curves, and are connected by no insertions or utricular vessels; but to all appearance have been formed by separate depositions of concreting matter. So the shells of snails and oysters are formed; their respective animals throw out periodically the osseous juice or testaceous matter, which adheres to the former shell and concretes, and thus successive layers are produced. In the same manner I suppose the concentric circles of the gorgonia to be formed, successive layers of juice exuding from the fleshy tubes that surround the hard part or bone of the animal. Thus the stem of the gorgonia *verticillata*, or Minorca white sea-feather, is composed of different layers of a shell-like substance, (see fig. 6,) where a broken part of the stem is represented, and a piece of it magnified, to show that there is evidently no more communication between the different laminæ than there is between those of an oyster-shell. In a transverse section of the gorgonia *pretiosa*, or true red coral, Donati has observed, *Philos. Trans.*, vol. 47, p. 97, [Abridgement, vol. x, p. 158.] "Different lines or annual

bands, whereof one part is of a rose colour, another yellowish, others white, others more or less charged with colours, that form concentric circles like the coats of an onion." This diversity of colours could hardly have taken place, had there been a circulation of juices through the stem; and it was probably owing to the different food which the animals had lived on at different periods.

There is another genus of zoophyte, which, though it swims freely about in the sea, yet approaches near to the gorgonia, and will serve further to explain the growth of its stem, and that is the pennatula, or sea-pen. This genus has a bone along the middle of the inside, which is its chief support. This bone receives the supply of its osseous matter by the same polype mouths, that furnish it with nourishment. Dr. Bohadsch has very judiciously brought to this genus the great Greenland clustered polype formerly described by me under that name, and now called pennatula arctica. In a cross section of the bone, (see Philos. Trans., vol. 48), the several laminae are magnified, to show that they are formed in layers like shells, and are not full of tubes as in a vegetable growth. These animals are ranged among the vegetating kind, and so called by Dr. Pallas. There is a great affinity between the gorgonia and isis, so that the increase of the bone of the latter will greatly illustrate that of the former. The longitudinal section of the bone to the stem of the isis hippuris will show, that it has been increased in diameter by successive layers of stony matter that surround it; see fig. 7. In this instance we can trace the bone in its infant state, when nature had given it pliable black horny joints, that it might yield the better to the violence of the waves; but as soon as it became stronger, these horny black joints were no longer necessary, as we find the lower part of the stems totally overgrown with the bony substance. The furrows in this coral are deeper than those of any other; inasmuch that not only the longitudinal fleshy tubes that surround the bone, but even the minute pores in them, through which the osseous juice exudes, are very discernible; see fig. 3.

We now come to a very singular circumstance in the growth of the gorgonia, in which it differs remarkably from that of trees. Fig. 8, is the figure of the naked stem or bone of a gorgonia, to which we find several tree oysters and other shells have adhered. These shell fish seem to have killed the gorgonia; for the same stem seems to be covered over with another gorgonia of the same kind; which in its growth has almost covered the shells, and likewise the branches to which they were fastened, leaving only part of the ends of the branches of the first gorgonia yet uncovered. The size and weight of the shells probably gave the waves so great a power over the stem, that it was at last broken off, and cast on shore in the state in which it is here represented. This instance of a gorgonia growing over one of its own kind, seems sufficient to account for the circle of calcareous matter found now and then in the cross sections of old stems,

between the horny circles, as has been observed by Dr. Pallas, Elench. Zooph. p. 162. "Interjecto quandoque tenui materiæ calcareæ strato." But I believe no one has ever seen the bark of trees inclosed in the same manner in the inner circles of the wood; and indeed it is so contrary to the laws of vegetation, that Dr. Pallas has not attempted to account for it, by showing any parallel instances in the transverse sections of timber. There is another remarkable instance of the manner of growing of these animals, in which the upper part of the gorgonia flabellum, meeting with an obstruction in growing upwards, grew downwards over its own fleshy substance, and evidently inclosed and covered over its own reticulated branches, with a continuation of its own flesh and bone. Dr. Pallas, in a note on the growth of the gorgonia, has the following extraordinary observation, that a gentleman in Holland is possessed of a gorgonia, which has on the same shrub, the bark partly of a gorgonia verrucosa, and partly of the gorgonia coralloides, without any visible difference of the branches; which he accounts for by comparing it to the growth of vegetables, saying: "So different lichens are often found incorporated in such a manner together, that they might easily be mistaken for one and the same plant." But I think it rather paradoxical to suppose the flesh of one animal to grow on the bones of another. If he examined it attentively, he would have found what we have advanced to be the case. It is not unusual for a gorgonia of one species to grow on the decayed branches of an individual of another, where the soft or fleshy part is already perished; but the upper or living gorgonia must have its own hard as well as soft parts; for should there be the fleshy part, and not the bony part, it would belong to the genus of alcyonium, and occasion such another remarkable mistake as this author has already made in his sertularia gorgonia, see Elench. Zooph. p. 188, where he has described an alcyonium, growing on and surrounding the stem and part of the branches of the sertularia frutescens, as a new species of sertularia. This, he says, most closely unites the genus of gorgonia with that of the sertularia; and to convince me of the truth of what he asserts, he has sent me part of the original specimen, of which fig. 9, exhibits an exact representation. At A is a magnified figure of this alcyonium, on a piece of the branch of the sertularia. It is of a fleshy substance with warts, having each 12 rays; we have many species of alcyonia from the West-Indies not much unlike this. The reader, by attending to the Doctor's own description of his sertularia gorgonia, will soon be convinced of the error, especially when he considers, that the character of a sertularia is that of a branched animal, with the hard parts without, and the fleshy parts within; and that the gorgonia, on the contrary hath its fleshy or soft parts without, and its bone or hard parts within.

There is another essential difference hitherto unnoticed, between the growth

of the gorgonia and that of trees; and that is, in the connection between the side branches and stem of the one, and the side branches and stem of the other. The side branches of vegetables proceed from the pith; of course, when a stem and side branch is divided lengthwise, the pith is seen continued through the main stem into the branch; see fig. 10, where *A* is the natural size of a small twig of the lime tree, and *B* the same magnified. It must be observed, that in some trees the channel or continuation of the pith, which leads from the stem to the side branch, is very much contracted, and the communication very narrow; in which case it will be necessary to make cross sections, which will soon discover the course of the pith from one to another. M. Du Hamel, an author of the first reputation, has clearly demonstrated this in his *Physique des Arbres*, vol. 2, p. 119, tab. 2, f. 91. Now in the gorgonia, the support, or what is called the woody part, is indeed furnished with a kind of a pith or medulla: but when we cut the stem or branch through the middle lengthwise, we find no passage whatever between the pith of the stem and that of the branch, each being surrounded with a hard covering of its own, which has no perforation, nor admits of any communication. Every branch of a gorgonia therefore has its own pith or medulla peculiar to itself, which is never found passing into that of another, see fig. 11, *A*, the natural size, *B* magnified. Again, in trees, the pith is largest in young shoots, and disappears in old stems: in the gorgonia the medulla is of the same diameter in the old stems as in the young branches. In the longitudinal sections of fresh shoots of trees, the pith in the microscope looks like a number of jointed tubes united together; and in the cross sections, it appears like so many circles. In dried specimens the tubular appearance in the longitudinal sections is more irregular; they look rather like longitudinal ranges of little transparent blebs, and the cross sections appear like circles intersecting each other in the margin; but there are many varieties of figures in the pith of different vegetables; what is mentioned here, is the common appearance of pith in most plants. When we cut a dry branch and stem of a gorgonia through the middle lengthwise, the pith appears divided into many little transverse membranes, like small white diaphragms, separated from each other about the distance of their own diameter. But these cross membranes are found to be more numerous in such as have been preserved directly from the sea in spirits; and when they are examined in the microscope, they appear to be of the nature and substance with the laminæ that compose the horny tube that surround them.\*

\* While comparing the longitudinal sections of the young branches of trees with those of the gorgonia, I was surprized to find such a similitude between the pith of a branch of a walnut tree, of a year's growth, and that of the gorgonia, see Grew, *Anat. of Plants*, tab. 19, fig. 4, *A* and *B*; especially as we are told by a modern author, who has published many microscopical observations on the



I come now to the outside covering or skin of the animal. As few have been at the pains to examine the surface of the gorgonia accurately, it has scarcely yet been noticed, that they are clothed with a kind of scales, and some of them so remarkably covered, and the scales so well adapted to the particular parts, that one might reasonably be induced to think, that nature has given them this defence, as she has done in like manner to the several parts of snakes and lizards, as a kind of armour to protect them from external injuries. As instances of the above, I shall only mention, that the surface of the stem, as well as the mouth of the cells of the gorgonia placomus, are defended by long pointed scales; see Essay on Corallines, p. 27, t. A, 1 to 3; and the gorgonia verticillata (of which an elegant specimen is to be seen in the British Museum) has also very remarkable scales of different sizes round the mouths and on the skin; see Essay of Corallines, t. 26, f. s, r. The gorgonia lepadifera has a most remarkable variety, placed like tiles, one over another, for the defence of the mouth of the cells that inclose the polype suckers; besides, there is a small kind of scales, that covers the surface of the stem and branches; see fig. 12.

From the skin we are naturally led to speak of the flesh of the gorgonia, or what the modern naturalists call the bark or cortex. Whoever has examined the flesh of the gorgonia, well preserved at the sea-side in spirits, will find, on dissecting them, proper muscles and tendons for extending the openings of their cells; for sending forth from thence their polype suckers in search of food; for drawing them in suddenly, and contracting the sphincter muscles of these starry cells, in order to secure these tender parts from danger; and also that there is, as we have already mentioned, proper secretory ducts, to furnish and deposit the osseous matter, for the supply of the bone, both of the stem and branches as well as the base, to secure its station with firmness, amidst the boisterous ele-

construction of timber, that the cell-like divisions in the branch of a walnut tree are only a row of single blebs of pith. But the microscope discovers to us, on viewing one of these cross membranes, that it is composed of many cells shrunk up and united together; for, on viewing the flat surface of one of them, it appeared full of circles intersecting each other, like a thick transverse section of many other dried piths pressed together: besides, the thicker part of this shrunk-up walnut pith, all round next the inside close to the wood, when magnified, plainly showed the same appearance of blebs as in other pith. To confirm this observation, May 23, 1772, I procured a young green shoot of a walnut tree, growing from a branch of the preceding year; and examining the pith, both in upright and transverse sections of this new shoot, I found that they exactly resembled the pith of many other trees, but were full of sap: and that the ranges of cells or blebs, that occupied one of these spaces, could not be less than 100, perhaps double that number of blebs. Dr. Grew takes notice, p. 120 in his Anatomy of Plants, that there are other trees, besides the walnut tree, whose pith in the last year's shoot shrinks up and forms such cavities; and an ingenious friend of mine, now engaged in an inquiry into the structure of plants, has shewn me a last year's stem of the brassica sylvestris, or shrubby cabbage, whose pith is shrunk and divided into a single row of cells, like those of the walnut tree of last year's growth.—Orig.

ment where it is appointed to be. That there are ovaries in these animals is without doubt; for in most of those that were sent to me preserved in spirits, the eggs were very visible on making longitudinal sections, in the same manner and form as in the *alcyonium digitatum*, called dead man's hand; see *Philos. Trans.*, vol. 53, Abridg. p. 41, vol. 12, but much larger; and it is very probable that many of these animals are viviparous, as we have seen among the *sertulariæ*.

So that I must conclude, that though they grow in a branched form, they are no more allied to vegetables than they are to the ramified configurations of *sal ammoniac*; to the elegant branched figures in the *Mocha* and other gems, called *dendrites*; to the *arbor Dianæ*, or the *arborescent* figures of the *Cornish native copper*: consequently, that animal life does not depend on bodies growing according to a certain external form. Hence it appears, that this metamorphosis of a plant to an animal is a flowery expression, and in my opinion, better suited to the poetical fancy of an *Ovid*, than to that precise method of describing which we so much admire in a natural historian.

*II. The Variation of the Compass; containing 1719 Observations to, in, and from, the East-Indies, Guinea, West-Indies, and Mediterranean, with the Latitudes and Longitudes at the time of Observation. The Longitude for the most Part reckoned from the Meridian of London. By Mr. Robert Douglass.* p. 18.

It is unnecessary to repeat these observations, as they, with many thousand other observations, have been employed by *Messieurs Mountaine and Dodson*, in constructing their universal magnetical charts.

*III. Propositions selected from a Paper on the Division of Right Lines, Surfaces, and Solids. By James Glenie, A. M., of the University of Edinburgh.* p. 73.

PROP. 1. *If from the angles at the base of any right-lined triangle, right lines be drawn to the alternate angles of rhombi, described on the opposite sides, and applied reciprocally to the sides produced; and from the vertex, through the intersection of these lines, a right line be drawn to meet the base: the segments of the base, thus made, will have to each other the duplicate proportion of the sides.—* Let *ACB* be any right-lined triangle, fig. 11, pl. 13. Let *AFEC*, *CDGB* be rhombi; on any two sides *AC*, *CB* of this triangle, applied respectively to *CB*, *AC* produced: from the alternate angles *EFA*, *DGB*, of which let *FB*, *GA*, be right lines drawn to the angles at the base, or third side, *AB*. Then, if through the intersection *o* of these lines, a right line *COL* be drawn from the vertex *c* to meet the base *AB*; the segments *AL*, *LB*, of the base thus made, will have to each other

the duplicate proportion of the sides AC, CB. This Mr. G. demonstrates geometrically, and then adds the following corollaries.

*Cor. 1.* If the triangle be isosceles, the right line drawn from the vertex to the base is perpendicular to it, and the segments of the base are equal to each other.

*Cor. 2.* When the triangle is right-angled, the line drawn from the vertex to the base is always perpendicular to it (as appears from E. 8, 6, and its cor.); and the rhombi become squares on the sides comprehending the right angle.

*Cor. 3.* The segments of the sides adjacent to the base, are respectively 3d proportionals to the sum of the sides, and the sides themselves.

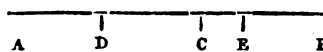
*Cor. 4.* The segments of the sides adjacent to the vertex are equal to each other, and each of them is a 4th proportional to the sum of the sides and the sides themselves.\*

*Cor. 5.* The segments of the base are proportional to the segments of the sides, which are adjacent to them.

*PROP. 2.* Let there be any two right lines given: there is an angle which may be made by these lines, such, that if from their extremities which do not meet, right lines be drawn to the alternate angles of rhombi described on them, and reciprocally applied to them when produced; and from the said angle through the intersection of these lines, a right line be drawn to meet the right line joining the said extremities; the segments of this line thus made, shall be respectively equal to the adjacent segments of the given lines.—Let AC, CB, be any two given right lines, fig. 12, pl. 13; and let CD, in AC produced, be equal to CB. On AD describe a semicircle; draw CN at right angles to AD, and equal to CD; join A, N, and apply a right line AM in the semicircle equal to AN. From the point M draw the right line MS at right angles to AD. Make a triangle ACB, having its sides equal to AC, AS, and CB; and ACB is the angle required to be found; and the segments AL, LB, of the right line AB joining the extremities A and B, of the given lines, are respectively equal to the segments AP, BQ, of the given lines, which are adjacent to them. This Mr. G. demonstrates as before.

*PROP. 3.* To multiply the square of a given finite right line by any number.—On an indefinite right line AP set off the given right line AB, fig. 13, pl. 13; draw BC at right angles to AP and equal to AB; and from A through C draw an indefinite right line AQ. Take AD equal to AC, and draw DE parallel to BC; AF equal to AE, and draw FE parallel to BC, and so on. Then it appears, from

\* And it may be added, a mean in proportion between the two segments adjacent to the base. For if a right line AB be any how divided in C, and from the two segments CA, CB, 3d proportionals to the whole line and each segment respectively, CD, CE, be taken away, the remainders AD, EB, are equal, and each is a mean in proportion between the two CD, CE.—Orig. S. HORSLEY.



47 E. 1, that the square of AC is equal to the square of AB multiplied by 2; the square of AE equal to the square of AD or AC multiplied by 2; that is, equal to the square of AB multiplied by 4, and so on. Thus the squares of AC, AE, AG, AI, &c. are respectively equal to the square of AB multiplied by the terms of the following series 2, 4, 8, 16, 32, &c. where the 63d term gives the square of AB multiplied by the last term of Sessa's Series for the Chessboard.

If CX be drawn parallel to AP, the squares of Aa, Ab, Ac, Ad, &c. will be respectively equal to the square of AB multiplied by 3, 5, 9, 17, 33, 65, 129, &c. Also if Ag be taken equal to Aa, and ge be drawn parallel to BC, and this be repeated, the squares of Ae, &c. will be equal respectively to the square of AB multiplied by 6, 12, 24, 48, &c. And the squares on Ao, &c. will be equal to the square on AB multiplied by 4, 7, 13, 25, 49, &c. In like manner, if AM be taken equal to Ab, and MN be drawn parallel to BC, the squares on AN, &c. will be equal respectively to the square on AB multiplied by 10, 20, 40, 80, 160, &c. And the square on AS &c. will be equal respectively to the square on AB multiplied by the terms of the series, 6, 11, 21, 41, 81, 161, &c.

In the same way, if right lines be drawn from E, e, G, N, I, &c. there will arise numberless other series. And if BC be taken equal to AB multiplied by any number, surd, fractional, or mixed, there will be obtained a great variety of series, consisting respectively of terms, which are surd, fractional, or mixed. And by dividing BC, DE, ge, FG, MN, HI, &c. in different ways, according to pleasure, we may apply the same method to fractional numbers, without altering the magnitude of BC. Thus, if BC be bisected, and a right line be drawn through the point of bisection parallel to AP, there will be found right lines, the squares on which are respectively equal to the square on AB multiplied by a great number of fractions, having 4 for their common denominator, and so on.

PROP. 4. *To find a right line, the square on which shall be equal to the square on a given right line, divided by any number.*—If, using the figure of the immediately preceding problem, we suppose the given right line to be denoted by AI, the squares on AH, AF, AD, AB, &c. will respectively be equal to the square on AI multiplied by  $\frac{1}{4}$ ,  $\frac{1}{2}$ ,  $\frac{3}{4}$ ,  $\frac{1}{2}$ ,  $\frac{1}{4}$ ,  $\frac{1}{8}$ ,  $\frac{3}{8}$ ,  $\frac{1}{4}$ ,  $\frac{1}{16}$ ,  $\frac{3}{16}$ ,  $\frac{1}{8}$ ,  $\frac{1}{32}$ ,  $\frac{3}{32}$ ,  $\frac{1}{16}$ ,  $\frac{1}{64}$ , &c. or divided by 2, 4, 8, 16, 32, 64, 128, 256, 512, 1024, &c.; and so on for other numbers, whole, surd, fractional, or mixed.

PROP. 5. *To cut off from a given right line a part expressed by any odd number.*—Let AB be the given right line, fig. 4. pl. 13. At right angles to it, at one of its extremities B, draw an indefinite right line BE. Let  $n$  be the given odd number, expressing the part of AB to be cut off. Take BC such a right line (prop. 3) that the square on it shall be equal to the square on AB, multiplied by the number  $\frac{n-1}{2}$ . Draw CL as in the first theorem, and take LS equal to AB. Then AS is that part of AB, which is expressed by the odd number  $n$ .

For the square on AC, being equal to the squares on AB, BC, is equal to the square on AB multiplied by the number  $\frac{n-1}{2} + 1$ , or  $\frac{n+1}{2}$ . Therefore it appears (from prop. 1 and cor. 1 to 20 E 6), that AL is to LB as  $\frac{n-1}{2} + 1$  to  $\frac{n-1}{2}$ . Consequently, AS is equal to the part required. Q. E. F.

Thus, if the square on BC be supposed successively equal to the square on AB multiplied by the terms of the series 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, &c. the numbers of the several parts denoted by AS, will be 11, 13, 15, 17, 19, 21, 23, 25, 27, 29, 31, 33, 35, 37, 39, 41, &c. which series comprehends all odd numbers after 9, and might have begun from 3, had the other series begun from 1.

PROP. 6. *To cut off from a given right line a part expressed by any even number.*—Let  $m$  denote any even number in general. Draw any indefinite right line BH, and at right angles to it another BE, fig. 15, pl. 13. On BE set off the given right line BA, and from A, with the distance equal to a right line, the square on which is equal to the square on AB multiplied by the number  $m - 1$ , intersect BH in some point C. From the vertex A of the triangle BAC draw AL as was directed in prop. 1, and draw LS parallel to CA. Then BL is such a part of BC as is expressed by the number  $m$ ; and BS is the same part of AB. Thus, if the square on AC be successively denoted by the square on AB multiplied by 3, 5, 7, 9, 11, 13, 15, 17, 19, 21, 23, 25, &c. then BS will be successively such a part of AB as is expressed by 4, 6, 8, 10, 12, 14, 16, 18, 20, 22, 24, 26, 28, &c.

PROP. 7. *If from the angles of the base of any right lined triangle, right lines be drawn to the alternate angles of rhombi described on the other two sides, and reciprocally applied to them produced; and through the intersection of these lines, a right line be drawn from the vertex to the base; the rectangle contained by the sines of the angles at the extremities of one of the sides, will be equal to the rectangle contained by the sines of the angles at the extremities of the other; and the parallelopiped contained by the sines of the angles of one of those triangles, into which the original one is divided by the said line drawn from the vertex, will be equal to the parallelopiped contained by the sines of the angles of the other.*

Cor. The two triangles, adjacent to the segments of the base, have to each other the proportion of the two adjacent to the sides containing the vertical angle, or the proportion of the two into which the original triangle is divided; and any one of these pairs of triangles are as similar figures described on the sides, being as the segments of the base, which have to each other the duplicate proportion of the sides.

PROP. 8. *If from the angles at the hypotenuse of any right angled right lined triangle, right lines be drawn to the alternate angles of squares described on the sides containing the right angle; and from the point where the right line drawn from the right angle, through their intersection, meets the hypotenuse, right lines*

*be drawn to the points where these lines meet the sides; the lines so drawn will make equal angles with the hypothenuse, and the right line drawn from the right angle to meet it; and will also have to each other the proportion of the sides containing the right angle.*

*Cor. 1.* The alternate triangles of those 4, which have their vertices in the point where the right line drawn from the right angle meets the hypothenuse, are similar, and have to each other the proportion of the segments of the hypothenuse, or the duplicate proportion of the sides containing the right angle.

*Cor. 2.* Either pair of the adjacent triangles lying on different sides of the right line drawn from the right angle, and having their vertices in the intersection of the right lines drawn from the angles at the hypothenuse, have to each other the proportion of the alternate triangles, having their vertices in the intersection of the first-mentioned line and the hypothenuse.

*Cor. 3.* The trapezium or quadrilateral figure formed by the segments of the sides adjacent to the right angle, and the right lines joining their extremities with the intersection of the hypothenuse and the right line drawn from the right angle to meet it, is capable of being inscribed in a circle; and is divided at the intersection of right lines drawn from the angles at the hypothenuse to the alternate angles of squares, described on the sides containing the right angle, into triangles which are proportional to each other, and when taken two by two, as they lie adjacent on different sides of the diagonal, are proportional to the unequal sides of the trapezium, and to the two triangles into which the diagonal divides it.

*PROP. 9.* *If from the angles at the base of any right lined triangle, right lines be drawn to the alternate angles of rhomboids described on the other two sides, and reciprocally applied to them produced; a right line drawn from the vertex, through the intersection of these lines, will cut the base into two parts, having to each other the proportion compounded of the proportion of the sides, and of the proportion of the other two lines comprehending the rhomboids.*—Let the triangle be  $ACB$ , the base  $AB$ , the rhomboids  $ACEF$ ,  $BCDG$ , fig. 11, pl. 13; and let the right lines  $BF$ ,  $AG$ , be drawn. Then, if from the vertex  $C$  through their intersection  $O$ , a right line  $COL$  be drawn to meet the base, the segments  $AL$ ,  $LB$ , will have to each other the proportion compounded of the proportions of  $AC$  to  $CB$ , and of  $CE$  to  $CD$ .

*SCHOLIUM.* If  $CE$ ,  $CD$ , be equal to each other, then  $AL$  has to  $LB$  the proportion of  $AC$  to  $CB$ , and  $CL$  bisects the angle  $ACB$ ; if  $CE$  have to  $CD$  the inverse proportion of  $AC$  to  $CB$ ,  $AL$  is equal to  $LB$ ; if  $CE$  have to  $CD$  the proportion of  $AC$  to  $CB$ ,  $AL$  has to  $LB$  the duplicate proportion of  $AC$  to  $CB$ ; and universally, if  $CE$  have to  $CD$  any multiply proportion,  $n$ , of  $AC$  to  $CB$ ,  $AL$  has to  $LB$  such a multiply proportion of  $AC$  to  $CB$  as is expressed by the number  $n + 1$ . And

if  $CB$  have to  $CD$  any multiply proportion  $m$  of  $CB$  to  $AC$ ,  $AL$  will have to  $LA$  such a multiply proportion of  $CB$  to  $AC$ , as is expressed by the number  $m - 1$ .

*IV. A new Method of Finding Time by Equal Altitudes. By Alex. Aubert, Esq., F. R. S. p. 92.*

Among the various methods practised for finding time, that by equal altitudes of the sun or of a star, has hitherto been esteemed the most eligible for observers, who are not furnished with a good and well-adjusted transit instrument. But this method, though one of the best, is generally attended with inconveniencies, which render the practice of it more difficult, and the result less perfect than could be wished. If the sun or stars near the equator be employed, as usual, and the altitudes be taken near the prime vertical, where the change of altitudes is the quickest, the interval of time between the observations must, in most latitudes, be of so many hours, that the observer cannot always attend to the corresponding altitudes: the weather may prove variable, so as to disappoint him at last; the clock or watch may go irregularly during so long an interval; and if the altitudes cannot, on account of their great distance from the meridian, be taken very high; an alteration in the state of the atmosphere may produce a variation of the refraction, and occasion the horary arcs to be different, though the apparent altitudes will be the same. To which may be added, the difficulty of making the instrument follow the object in its motion in azimuth, without danger of disturbing its adjustment in regard to altitude. To remedy all these inconveniencies, the following method was thought of; and having been practised with constant success, it is presumed the communication of it may be acceptable to astronomers.

If a star be selected, of which the polar distance is very little less than the complement of the latitude of the place of observation, it will, at equal distances from the meridian, come to vertical circles, which touch its parallel of declination. The star, when in these vertical circles, will be near the meridian, near the prime vertical, and near the zenith; and consequently, if it be observed there, the interval between the eastern and the western altitudes will be short; the alteration in altitude will be quick; the star cannot be affected by a different refraction; besides, it will have no motion in azimuth. To observe the star in these vertical circles, 2 things are necessary: the first is, to be provided with an astronomical quadrant, having 3 or more horizontal wires in the telescope, and if it has also a speculum at the eye-end of the telescope, to bring the vertical ray horizontal, it will be found very convenient. The next thing is, to make a computation of the apparent zenith distance of the star in the vertical circles which touch its parallel of declination; for if the telescope be fixed to

this zenith distance, as soon as the star is found to come to it, it will be in the proper vertical circle.

The advantage of this method will appear in the following example of equal altitudes, taken July 15, 1773, at Loam-pit hill, near Deptford, in latitude  $5^{\circ} 28' 7''$  N. and longitude  $5'$  in time w. of the Royal Observatory at Greenwich. The star selected was  $\gamma$  Draconis, having  $38^{\circ} 28' 21''$  apparent north polar distance, being very little less than the complement of the latitude  $38^{\circ} 31' 53''$ . Then,

As cos.  $38^{\circ} 28' 21''$  : rad. :: cos.  $38^{\circ} 31' 53''$  : cos.  $2^{\circ} 19'$  the zenith distance;  
and sin.  $38^{\circ} 31' 53''$  : rad. :: sin.  $38^{\circ} 28' 21''$  : sin.  $87^{\circ} 5' 20''$  the azimuth;  
also rad. : tan.  $38^{\circ} 28' 21''$  :: cotan. ...  $38^{\circ} 31' 53''$  : cos.  $3^{\circ} 43' 13''$  the horary arc =  $14^m 52^s.9$  in sidereal time, or  $14^m 50^s.5$  in mean time.

The true zenith distance being  $2^{\circ} 19'$  the same was diminished by  $2''$  for refraction, and the telescope fixed to  $2^{\circ} 18' 58''$ , the apparent zenith distance; and when the star came to the wires, the times by the clock were as follow:

Eastern altitudes.				Western altitudes.				Meridian passage.			
1st wire at.	9 <sup>h</sup>	55 <sup>m</sup>	43 <sup>s</sup>	2 <sup>m</sup>	14 <sup>s</sup>	2 <sup>m</sup>	14 <sup>s</sup>	10 <sup>h</sup>	29 <sup>m</sup>	46 <sup>s</sup>	10 <sup>h</sup> 12 <sup>m</sup> 44 <sup>s</sup> .5
2d .....	9	57	57	2	14	10	27	32	10	12	44.5
3d .....	10	0	9	2	12	10	25	20	10	12	44.5

so that in about  $34^m$  the complete set of altitudes was obtained near the prime vertical, free from the effects of a different refraction, and any motion in azimuth. The horary arc observed by the middle wire not turning out exactly according to the computation, is of no consequence to the observations. Some little difference may arise in it from small inaccuracies in the estimation of the star's apparent polar distance, the latitude of the place, or the error of the line of collimation; or from not setting the telescope exactly to the proper zenith distance; but as the chief intention of the computation is to find the vertical circles in which the star has no motion in azimuth, the other parts of it need not be strictly attended to.

The following manner of inferring mean time from the star's meridian passage, being more convenient and concise than the usual one, may also be acceptable. From the star's apparent right ascension, increased by 24 hours if necessary, subtract the sun's apparent right ascension for apparent noon; diminish the remainder by the proportional part of the star's acceleration, at the rate of  $3^m 55^s.91$  for 24 hours, of which a table is easily computed; to this last remainder apply the equation of time for apparent noon, according as it is additive or subtractive; the result will be the mean time of the star's passing the meridian.

Ex.—The AR apparent of $\gamma$ Draconis July 15, 1773, was.....	17 <sup>h</sup>	51 <sup>m</sup>	24 <sup>s</sup> .0
— the apparent AR of the sun at apparent noon.....	7	39	59.0
First remainder.....	10	11	25.0
— the star's acceleration for $10^h 11^m 25^s$ , at $8^m 55^s.91$ for 24 hours.	1	40.2	
Second remainder.....	10	9	44.8
+ the equation of time at apparent noon, additive .....	5	27.7	
Gives the star's meridian passage in mean time .....	10	15	12.5



But the clock showed  $10^h 12^m 44^s.5$  when the star passed, consequently it was  $2^m 28^s.0$  too slow for mean time.

Observers, who are not furnished with tables of the sun's right ascension, and of the equation of time for the apparent noon of their meridian, may apply both as they are given in the Nautical Ephemeris for the meridian of the Royal Observatory at Greenwich; the result will be the mean time of the star's passing the Greenwich meridian. And by applying the proportional part of the foregoing acceleration of  $3^m 55^s.91$ , belonging to the difference of longitude in time of the place of observation from Greenwich, the mean time of the star's passing the meridian of the place of observation will be found. If the place be to the east of Greenwich, the acceleration will be additive; if to the west, subtractive.

In a similar manner, the mean time of any observation made with a clock, regulated to sidereal time, may be inferred, provided the preceding transit of the sun has been observed; for if from the time of the observation by the clock, increased if necessary by 24 hours, the time of the observed transit of the sun be subtracted, the remainder, diminished by the proportional part of  $3^m 55^s.91$ , and duly corrected by the equation of time for the preceding noon, will give the mean time required. It is understood that the clock keeps the rate of sidereal time exactly; for if not, a further correction for the loss or gain since noon must be applied.

END OF VOLUME THIRTEENTH.

Fig. 1.  
Pa. 7.



Fig. 2.

*Sponges. Pa. 32.*



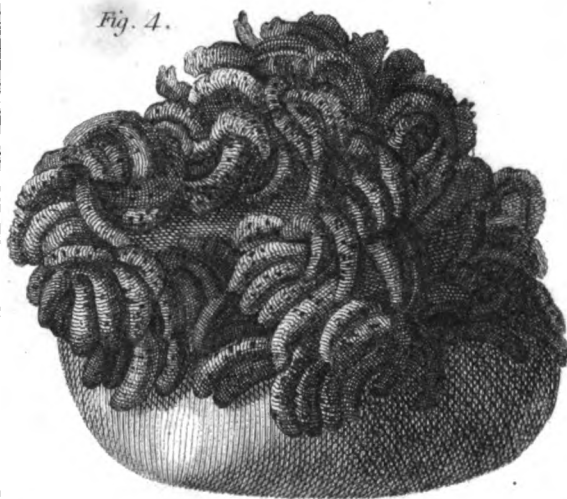
Fig. 5.



Fig. 3.

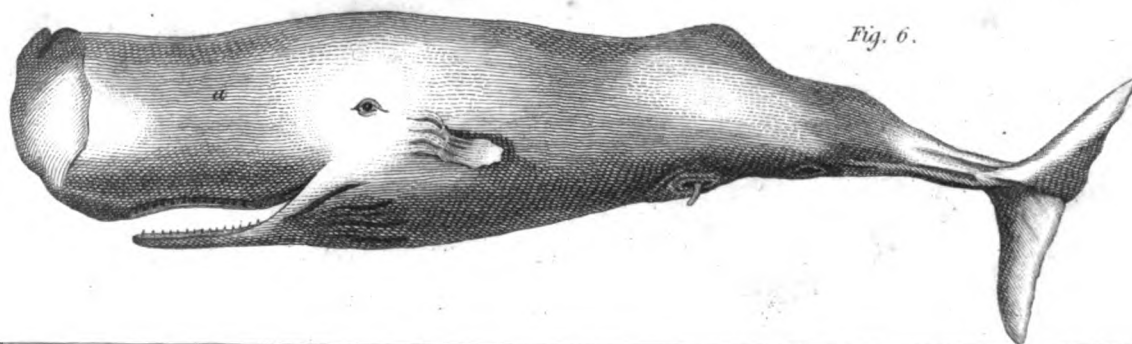


Fig. 4.



*Cachalot. Pa. 58.*

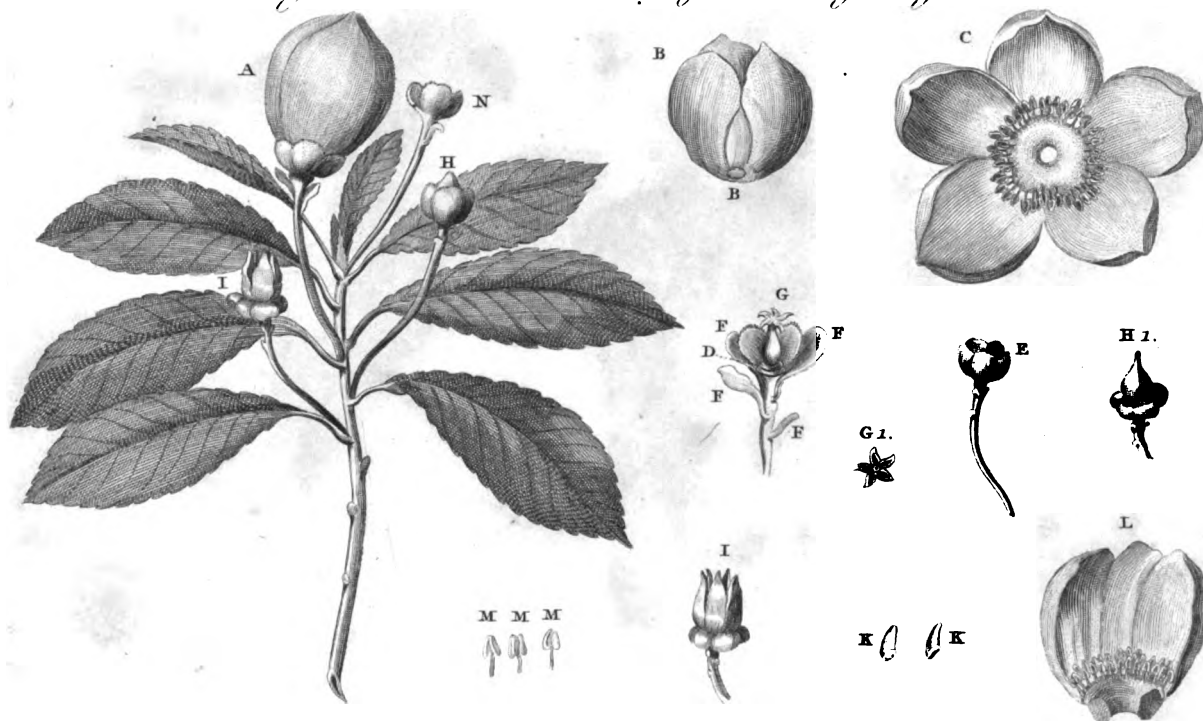
Fig. 6.



*Mutlow Sc. Russell Co.*



*Gordonia Lasianthus. Vulgo Lobolly Bay. Pa. 85.*



*Illicium Floridanum. Vulgo Starry Aniseed Tree. Pa. 87.*



Mulder Sc. Bap. 65



Fig. 1. p. 94.



Nyl-ghau—p. 117.

Fig. 2.



*The Bivalve Insect—p. 136.*

Fig. 3.

Fig. 4.



Fig. 6.

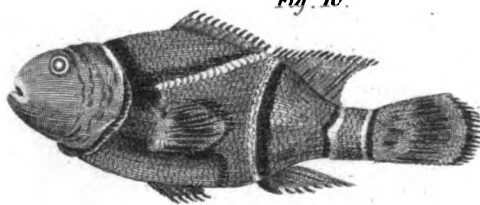


Fig. 9.



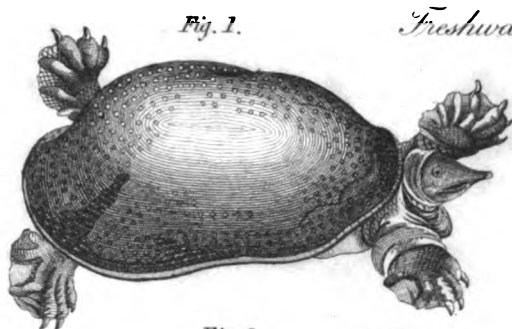
*Thoracic Fish—p. 136.*

Fig. 10.



Walter S. Russell Gt.





Freshwater Turtle. Pa. 144.

Fig. 2.

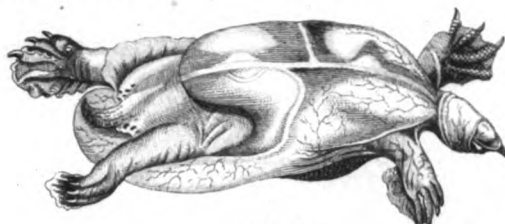
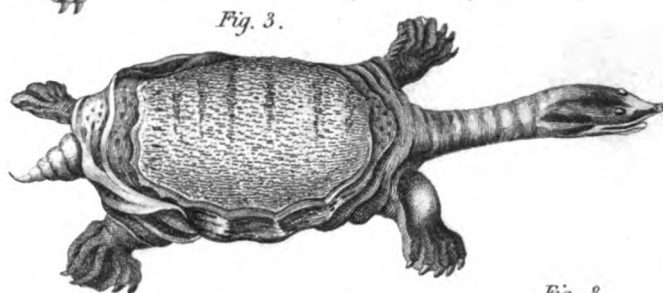


Fig. 4.



*Nyctanthes elongata*. Pa. 147.  
Half the natural size.

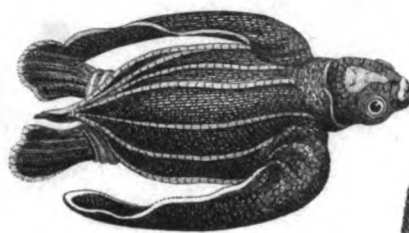


Fig. 5.



Fig. 8.

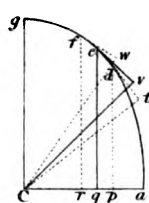


Fig. 7. pa. 150.

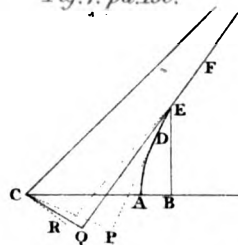
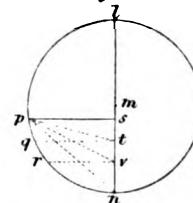


Fig. 9.



Appearances of Venus.  
by M<sup>r</sup> Cha<sup>r</sup> Green. Pa. 175. by Cap<sup>t</sup> Cook. Pa. 176.

Fig. 5.

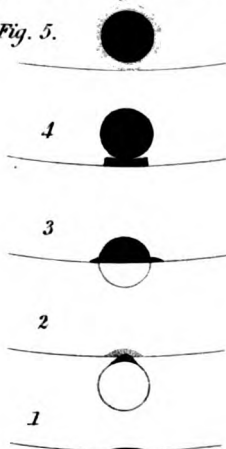
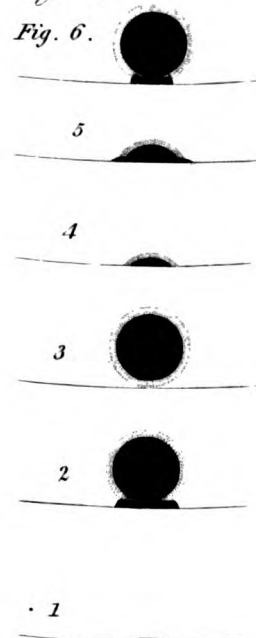


Fig. 6.

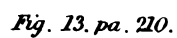
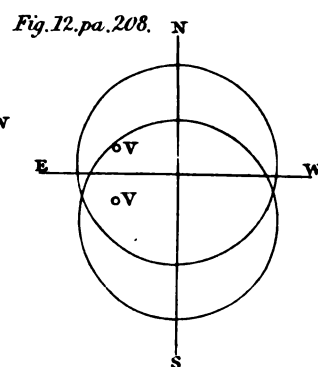
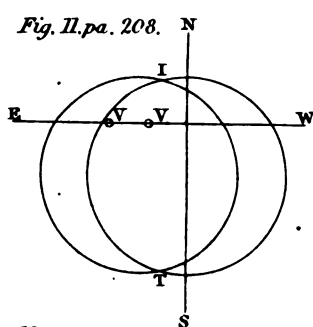
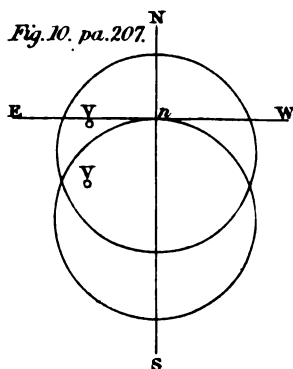
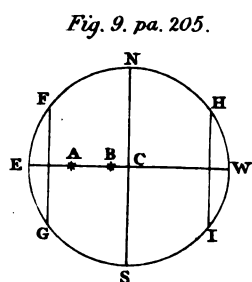
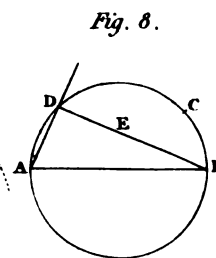
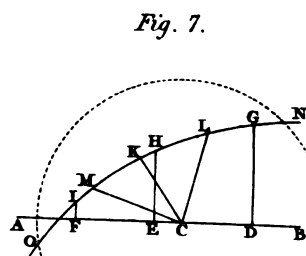
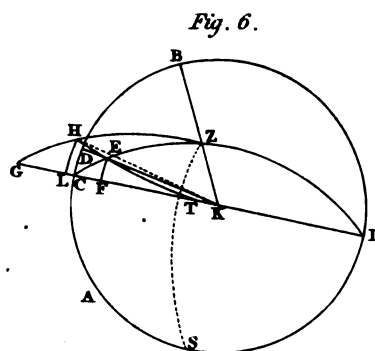
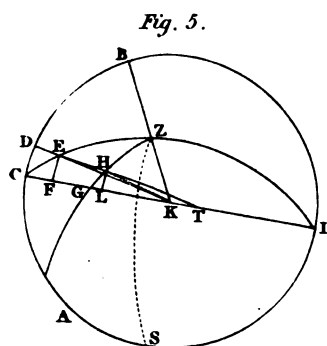
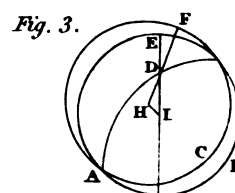
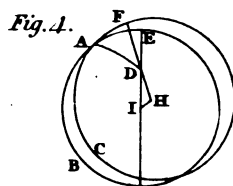
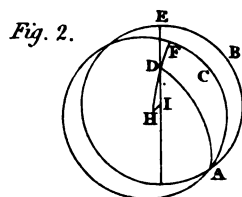
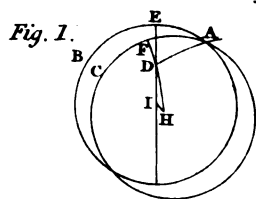


Mudlow &amp; Russell del.



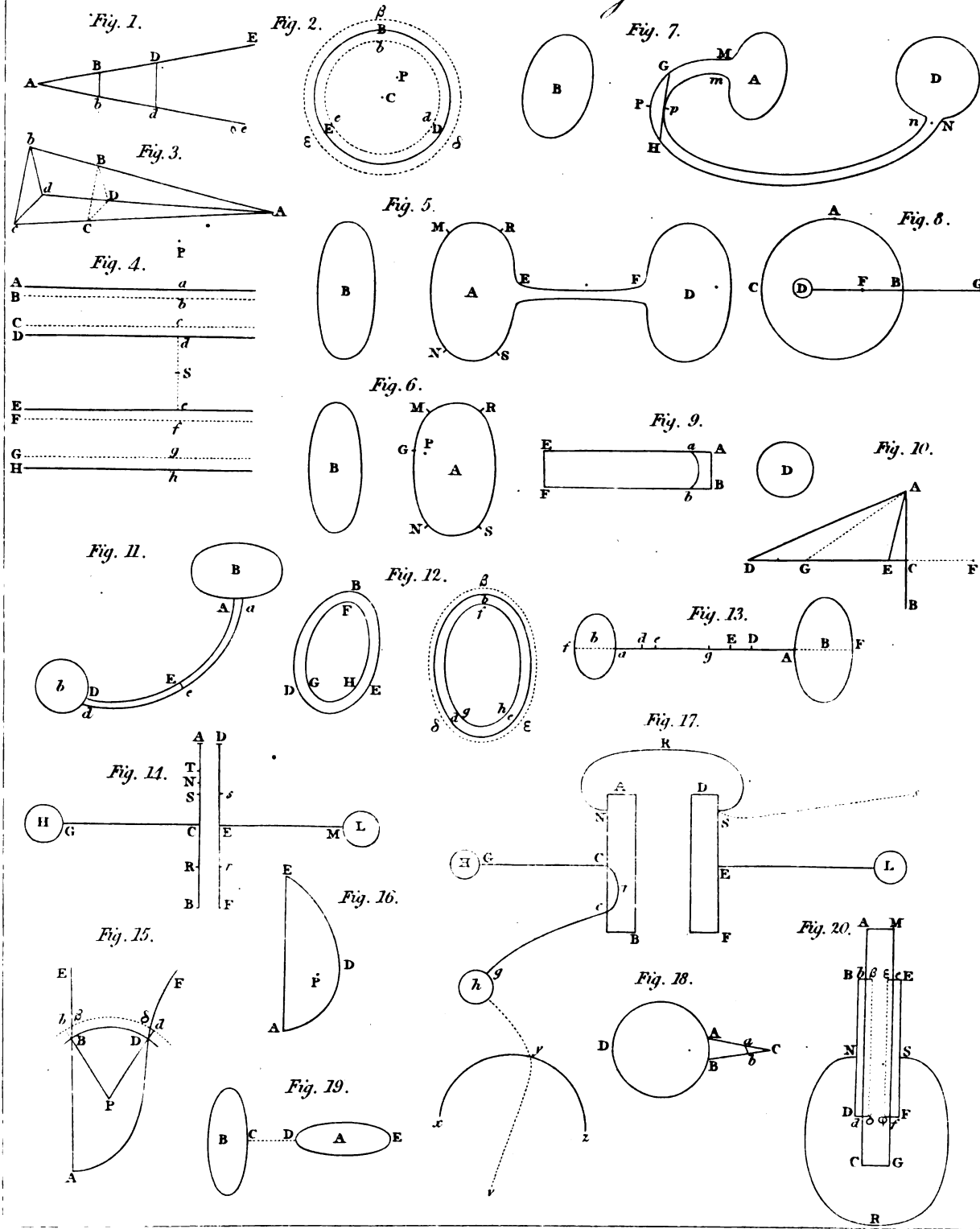


*Pemberton on Eclipses. Pa. 181. &c.*





*Mr. Cavendish on Electricity. Pa. 225. &c.*



*Mathew St. Bailett del.*





Fig. 2. Indian Zodiac. p. 321.

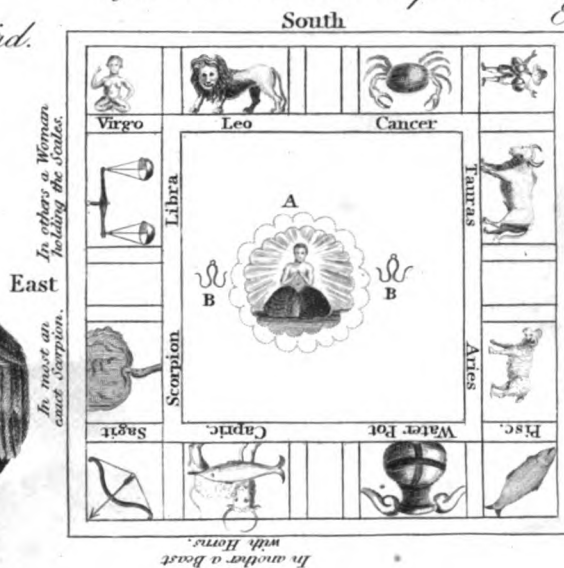
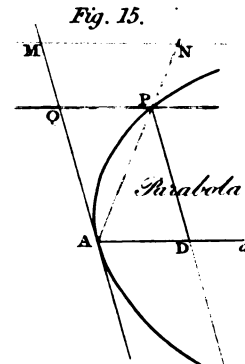
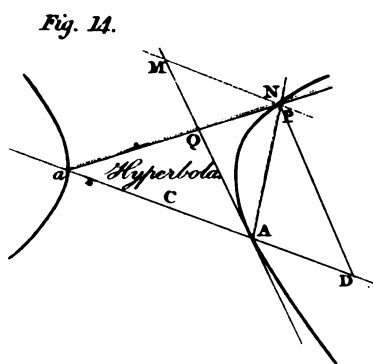
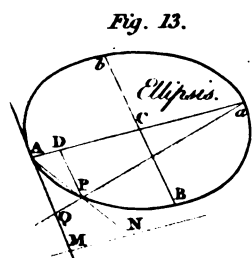
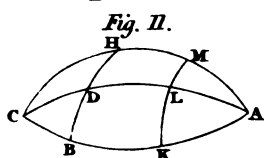
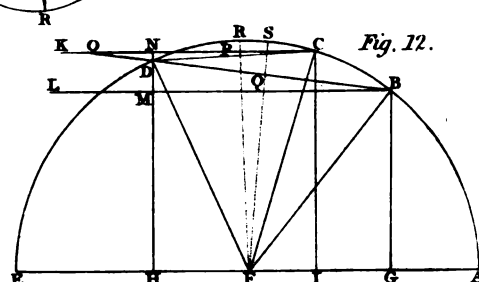
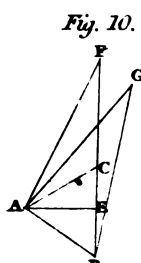
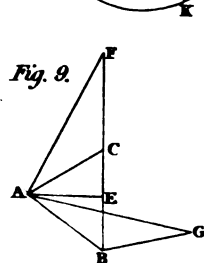
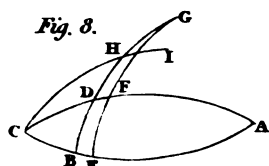
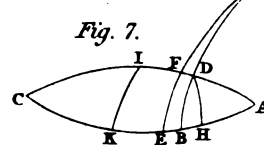
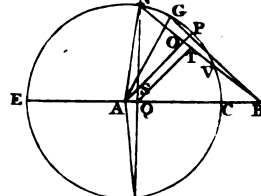
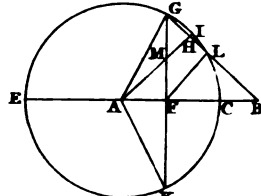
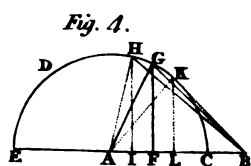
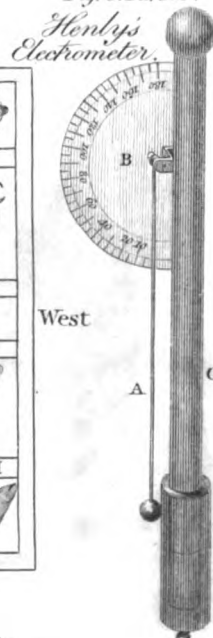


Fig. 3. Pa. 323.



Museum Sc. Reg. 22. 61



*Sea Anemonies. Pl. 460 &c.*

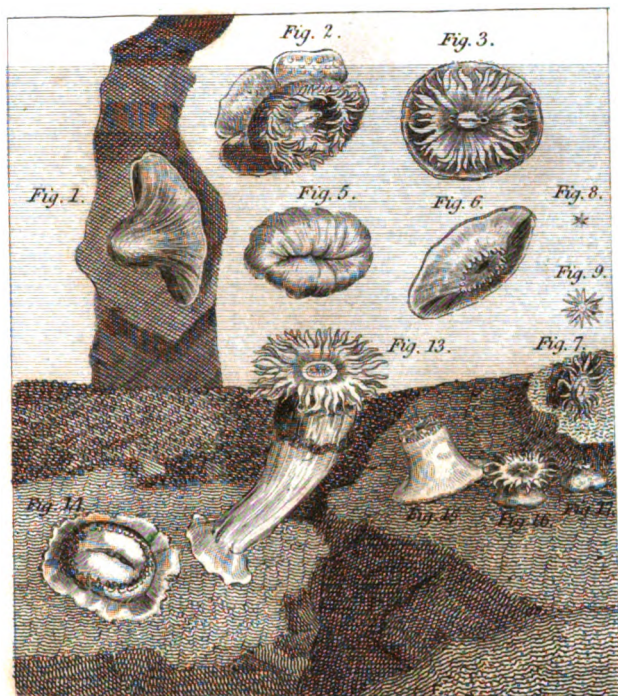


Fig. 10.

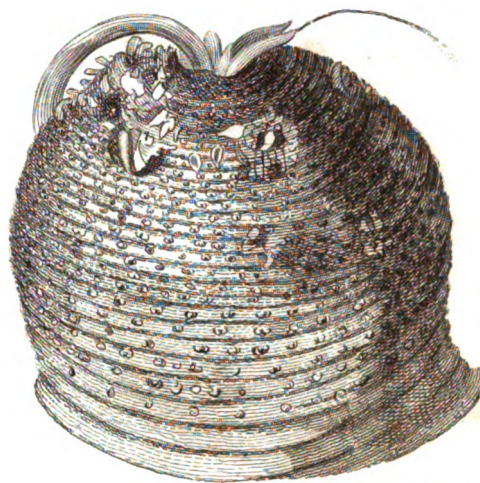


Fig. 11.

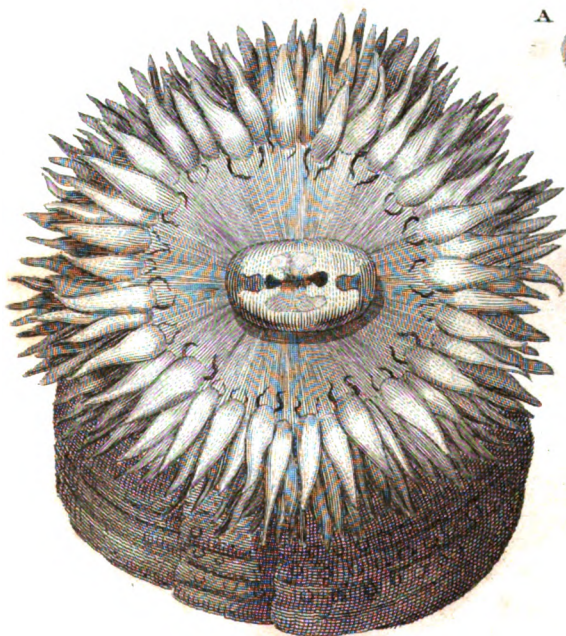


Fig. 12.



Mudlow Sc. Russell & Co.





*The Male & Female Torpedo, or Electric Ray, frequenting the Sea Shores in the Neighbourhood of La Rochelle. Pa. 477.*

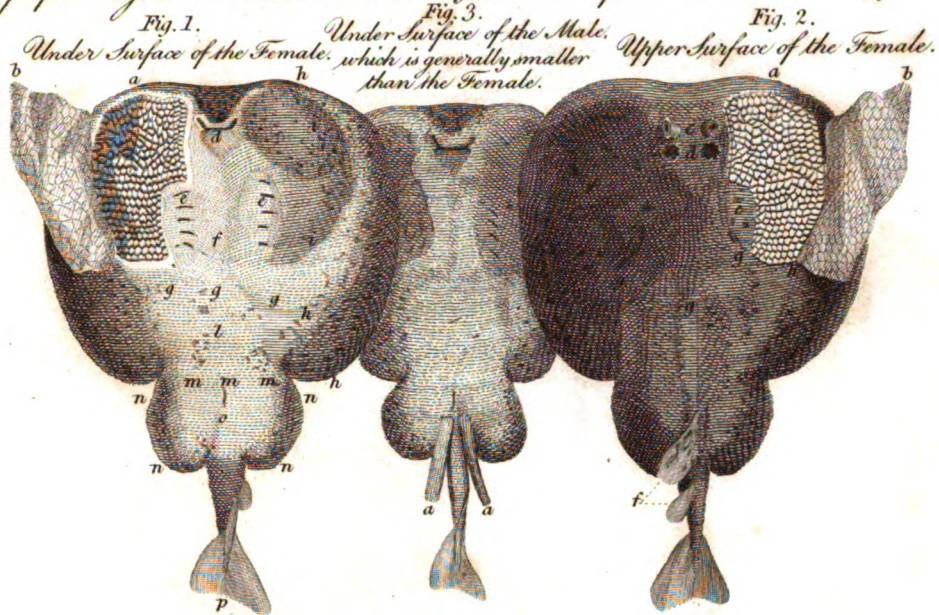


Fig. 4.

*Torpedo Electric Organs. Pa. 481.*

Fig. 5.

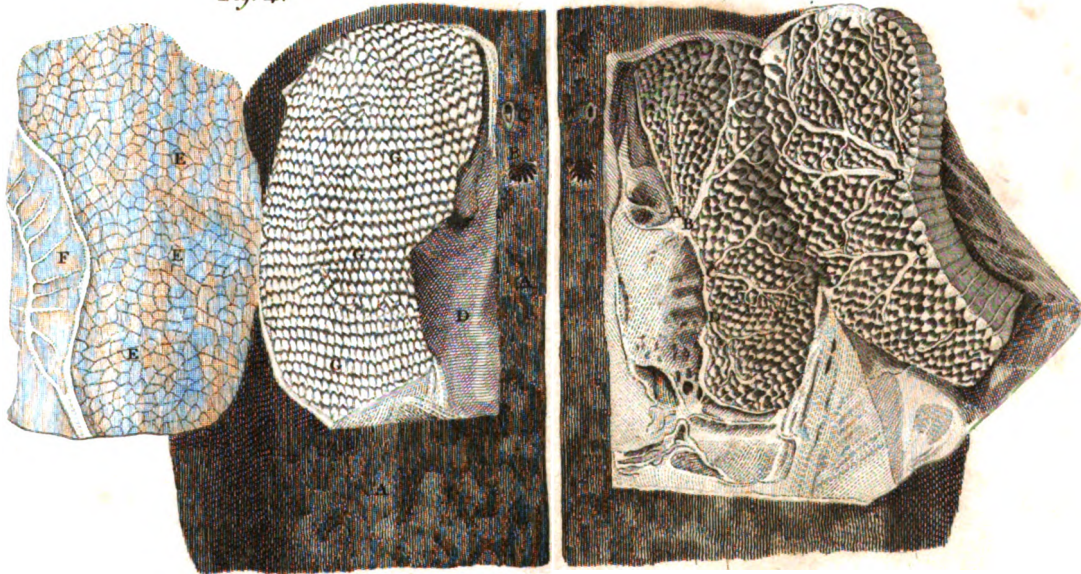
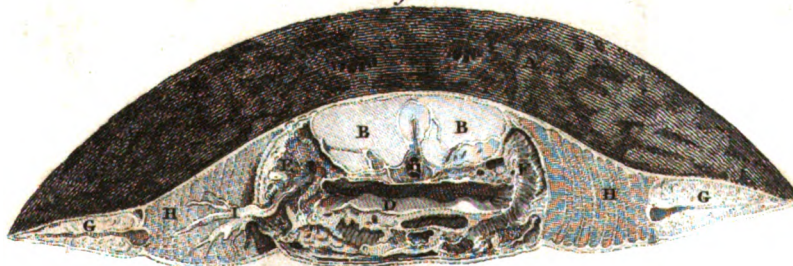


Fig. 6.



Mason & Russell del.



*Points and Balls.*

Fig. 1. Pa. 501.

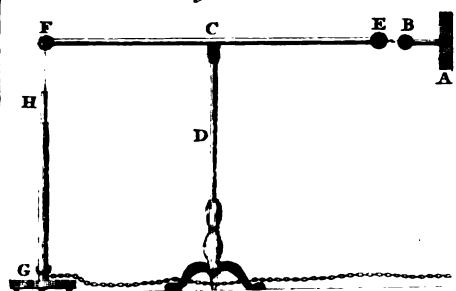


Fig. 2. p. 512.

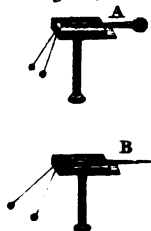


Fig. 3.

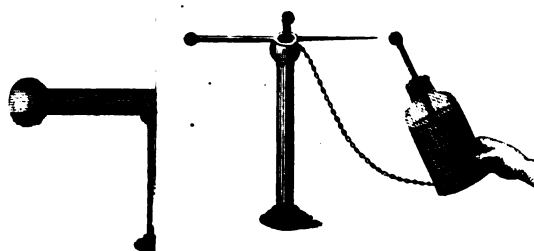


Fig. 4.

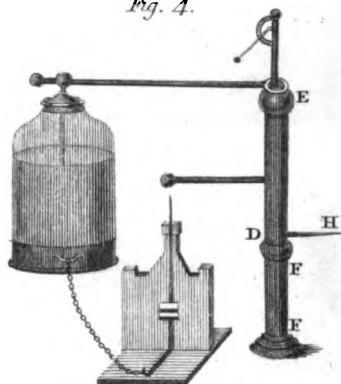


Fig. 5.

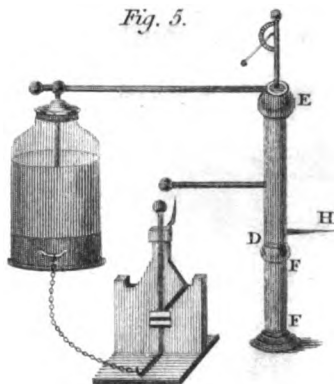


Fig. 6.

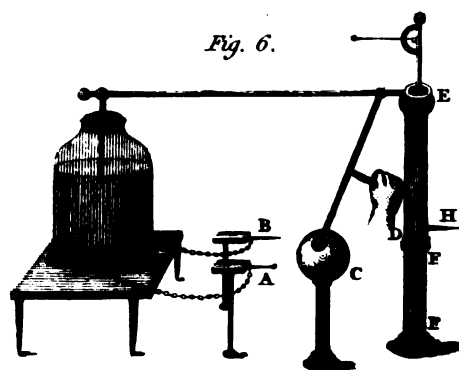


Fig. 7.

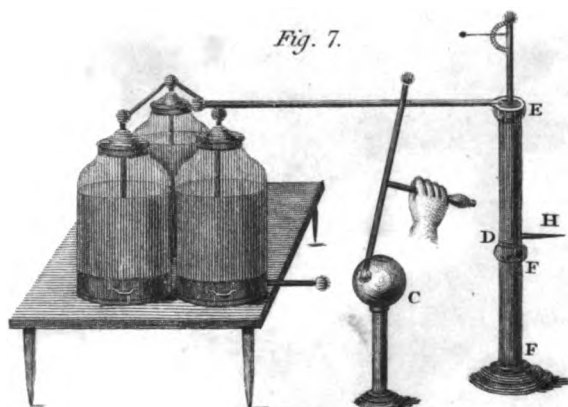
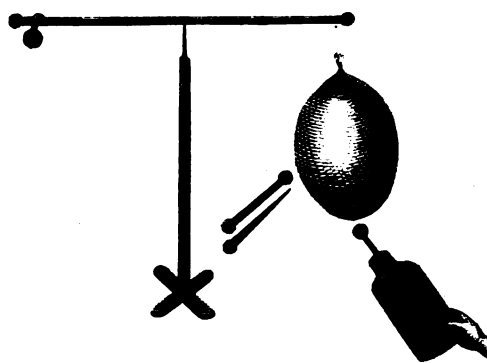
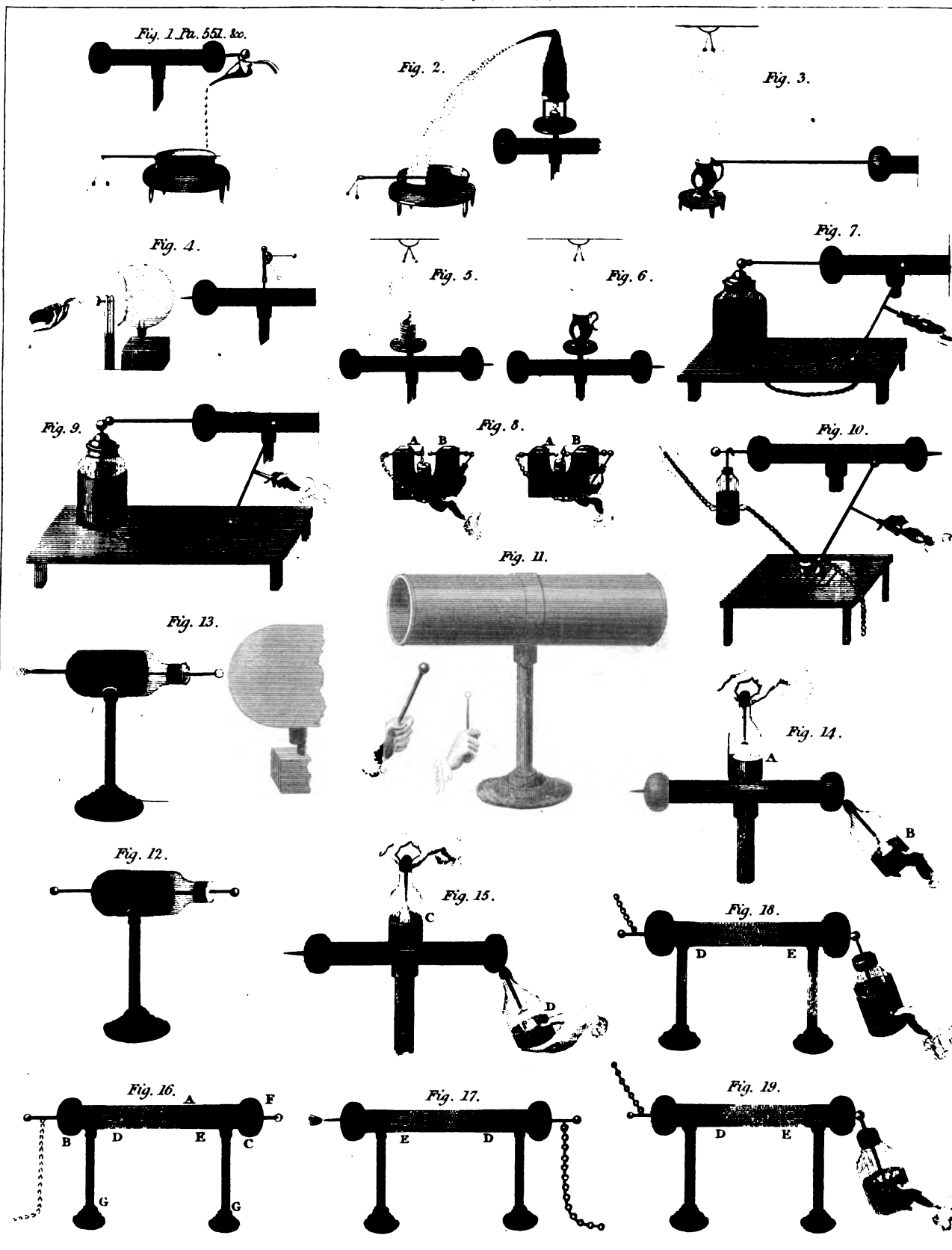


Fig. 8.



Muttow &amp; Co. Russell &amp; Co.





London: S. P. P. 1807.



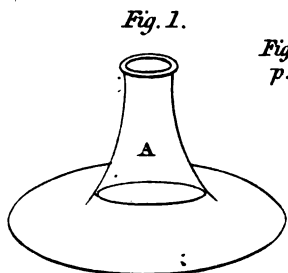


Fig. 1. to 8.  
p. 188.

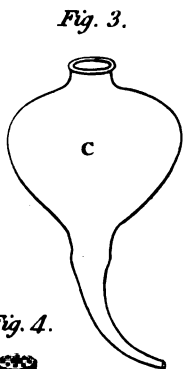


Fig. 6.



Fig. 7.



Fig. 8.

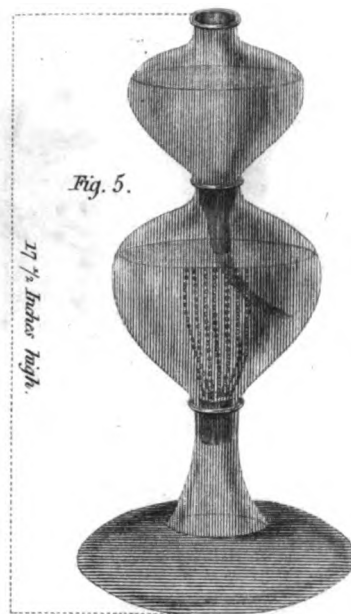


Fig. 2.

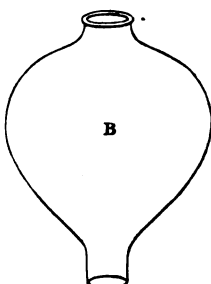
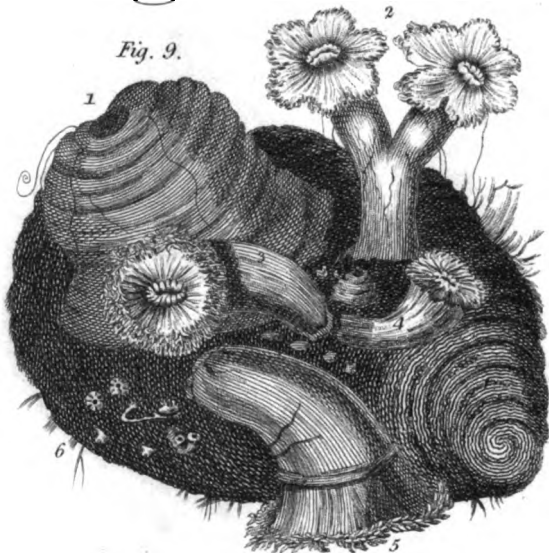


Fig. 4.



p. 643.

Fig. 9.



p. 645.  
Fig. 10.

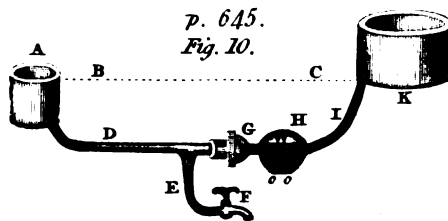


Fig. 15.

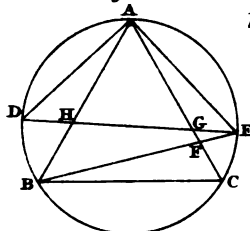
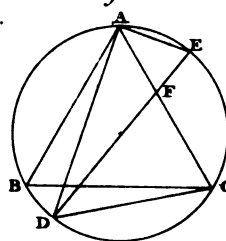
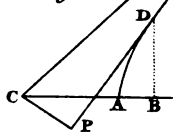


Fig. 16.



p. 651.

Fig. 11.



p. 647.

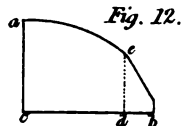
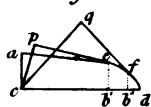


Fig. 13.



p. 647.

Fig. 14.

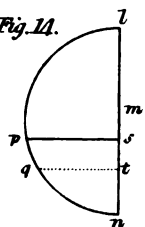
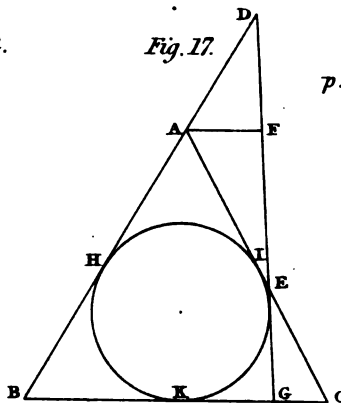
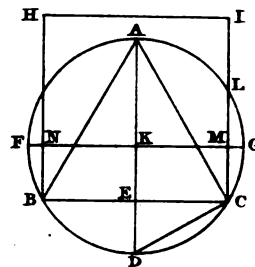


Fig. 17.



p. 651.

Fig. 18.



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Fig. 1. Pa. 653.

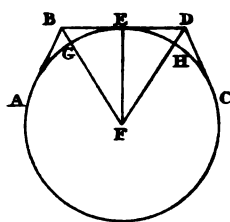


Fig. 2.

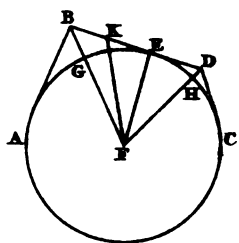


Fig. 3.

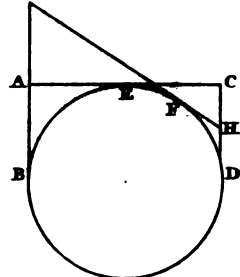
Fig. 4. Wind Gauge.  
p. 661.

Fig. 5.



Fig. 6.

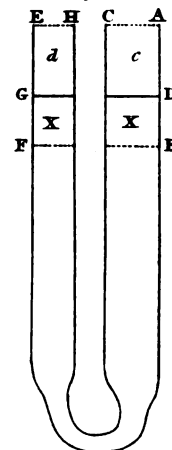


Fig. 7.

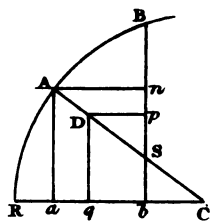
Fig. 7. to 10  
p. 690.

Fig. 8.

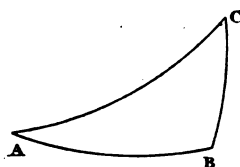


Fig. 9.

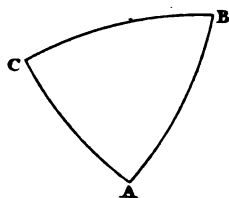


Fig. 10.

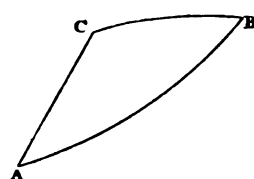


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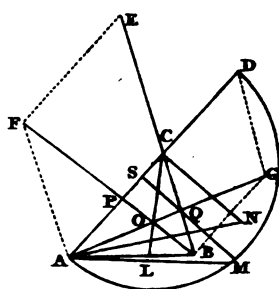


Fig. 13.

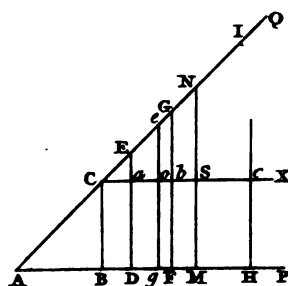
Fig. 12. to 15  
p. 729.

Fig. 14.

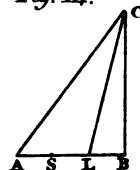
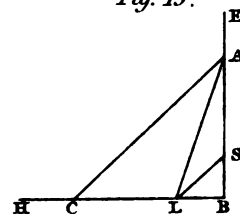


Fig. 15.

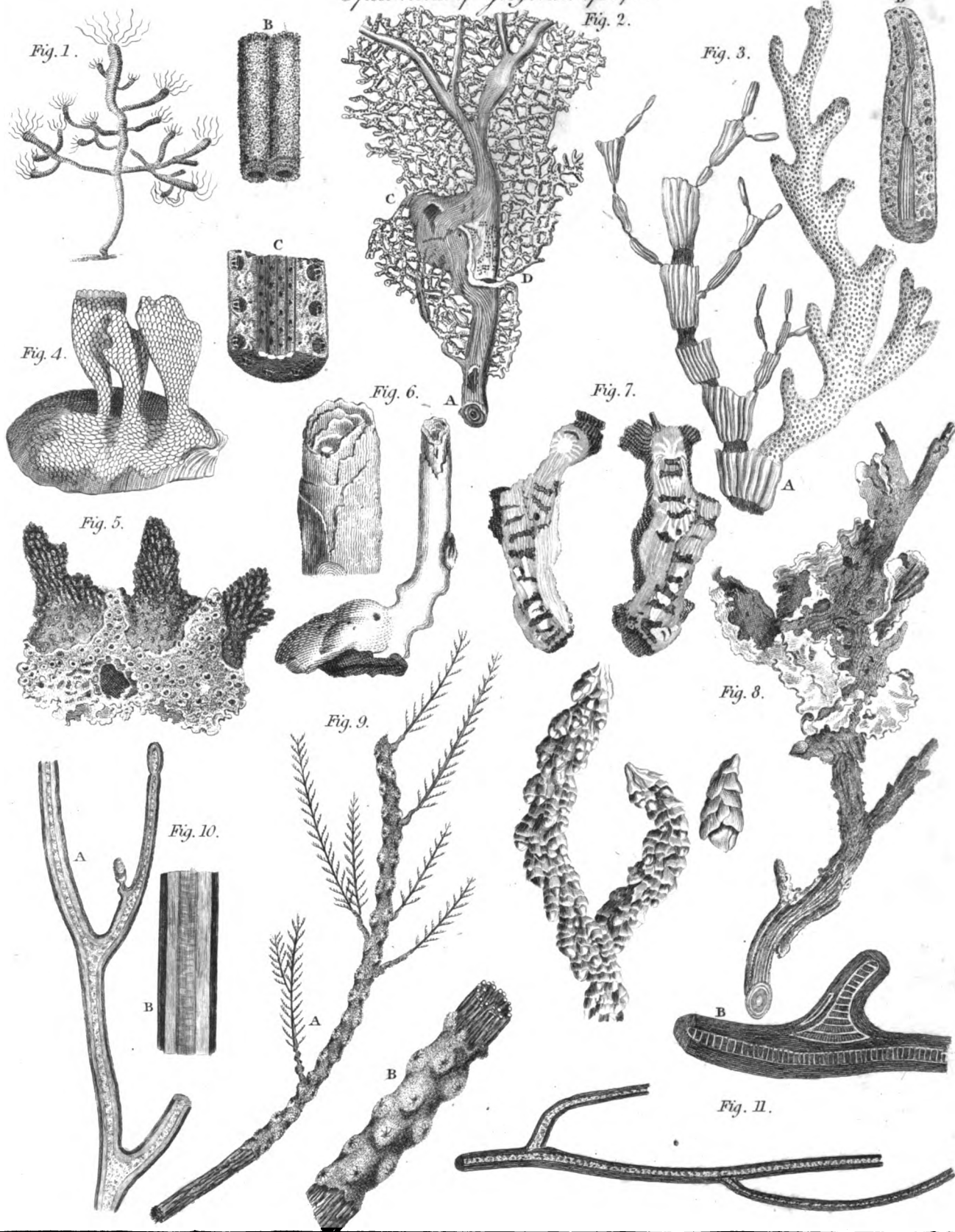


Mudlow Sc. Engrs. &amp; Co.



*Specimens of Gorgonia. p. 720.*

Fig. 2.



Mulrow Sc. Reg. coll. 61



Fig. 1.

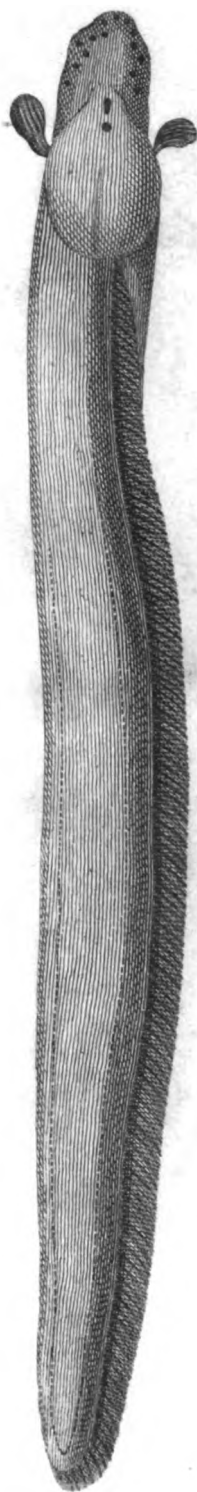


Fig. 2.

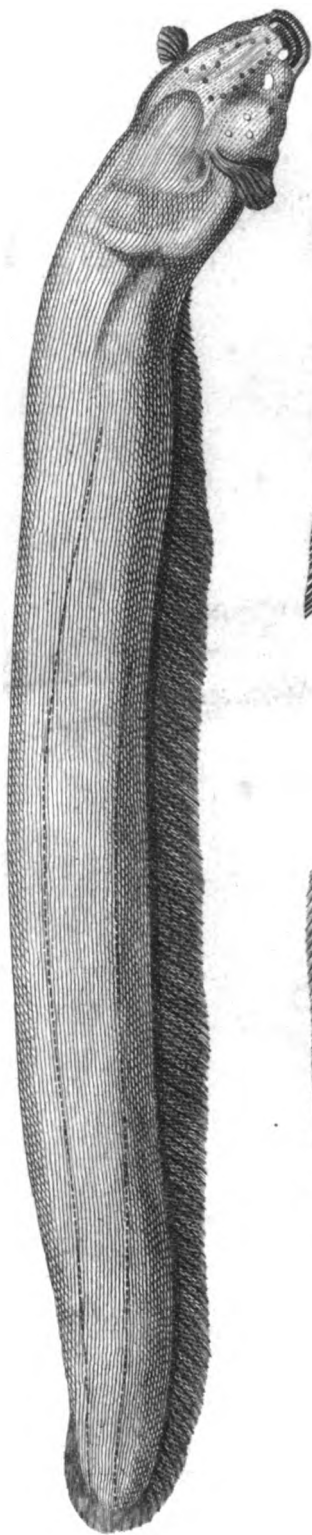
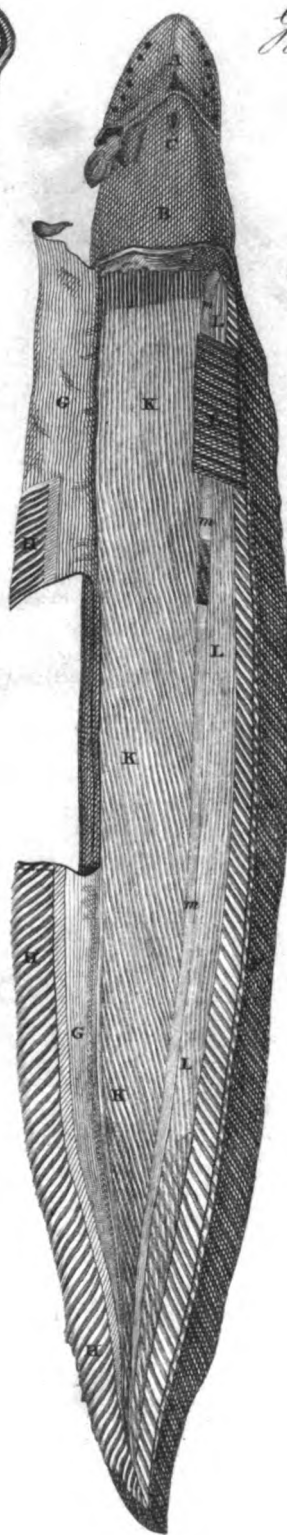


Fig. 3.



*Gymnotus Electricus. p. 674.*

Fig. 4.

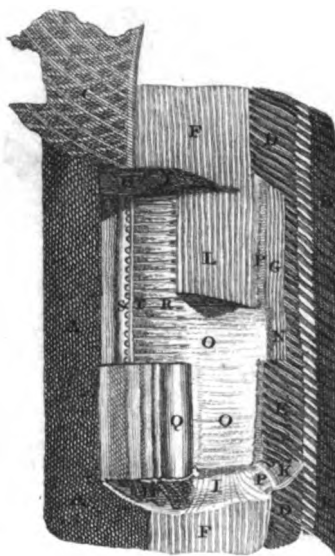
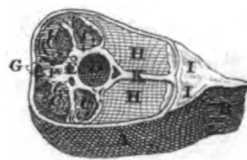


Fig. 5.



Muslow Sc. P. 17. 171.











